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COURSE OF LECTURES
ON THE
PHYSIOLOGY AND PATHOLOGY OF THE BLOOD,
AND
THE OTHER ANIMAL FLUIDS,
DELIVERED IN THE SESSION OF 1839-40,
AT THE
School of Anatomy adjoining St. George's Hospital,

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LECTURE VI.

The liquor sanguinis or plasma.—The coagulation of blood: consists in the separation of fibrin from the liquor sanguinis, and its concretion.—The clot, a mixture of serum, fibrin, and red corpuscles.—The less obvious phenomena attending coagulation.—Origin of the fibrin.—Causes assigned.—Vital power the true cause: coagulation takes place only so long as blood is living.—The ordinary influences on coagulation: air; form and material of the vessel; mass; agitation or rest; temperature.—Necessity of paying due regard to their effects.

GENTLEMEN:—We have already seen that the living blood, as it flows in its vessels, presents the appearance of *corpuscles* floating in a *transparent fluid*. My last lecture comprised a considerable part of the microscopical history of the former. I propose to call your attention to-day to the fluid in question, and to those vital changes which take place in blood after its extravasation, occasioning its separation into a vapour or *halitus*, a fluid called *serum*, and a more solid *clot*.

It is remarkable that so many years should have elapsed, and so many observations have been made, since Boyle first introduced the experimental method into our investigations upon the blood, and the microscopical discoveries of Malpighi and Hewson, before any very decided distinction was made between these two fluids. The living liquid was frequently meant when the word serum was employed by physiologists. Müller on the continent, and Dr. Babington in this country, were, I believe, the first to describe the former, under the term *liquor sanguinis*, and Schultz has employed the word *plasma*, to designate the same thing. For the purpose of illustrating the distinction between these two fluids, I shall repeat one or two simple experiments from Hewson's inquiry, and Dr. Babington's original paper.

Exp. 1.—Hewson abstracted blood from a patient labouring under *phthisis*. It flowed in a full stream, and soon filled the bason. The surface was then observed to become transparent, and the transparency went deeper and deeper, the blood being still fluid. The coagulation was observed to begin first on the surface, forming a thin pellicle in contact with the air, and the removal of this was soon succeeded by another. A part of the clear liquor was then taken up with a wet teaspoon, and put into a phial, with an equal quantity of water; a second portion was kept in the teaspoon, and both were found to coagulate, and to separate serum.

Dr. Babington repeated this experiment with blood from a patient suffering under acute rheumatism. He collected the liquor in quantity, and transferred it to another vessel. It became opalescent, somewhat viscid, and perfectly homogeneous in appearence. After standing sometime, it separated into two parts—a clot of fibrine, which had the precise form of the vessel in which it was gathered, and a clear fluid, with all the properties of serum. Hence it may be inferred that blood, as observed to be the case microscopically, has only two constituents—the corpuscles and the liquor sanguinis; and that in this and similar instances, it remains fluid long enough to permit the heavier corpuscles to subside, after which the fluid separates by a general and uniform coagulation and contraction into two parts, and a portion of the solid part through which the corpuscles have time to fall, forms a coating of pure fibrine, while the portion into which they descend forms the coloured clot.

To obviate the objection that the blood was diseased in these experiments, Dr. Ba-

bington placed the healthy fluid under conditions which enable its red corpuscles to settle more quickly than would otherwise happen, by

Exp. 2.—Take two similar jars, each capable of holding four or five ounces, and fill one of them with olive oil. Draw the blood of a healthy subject into them. That which flows through the oil, will have its coagulation retarded a sufficient length of time for the formation of a layer of liquor sanguinis on its surface, which will produce a buffy crust; there being none in the other jar. Hence, healthy blood is similarly constituted to that disposed to form a distinct fibrinous coating, the obvious difference being that the former coagulates more quickly than the latter.

I have already stated, that when the liquor sanguinis has separated a coagulum, its shape is precisely the same as that of the containing vessel. It has also a similar density throughout. We may, therefore, conclude that the coagulation takes place uniformly from every part of the fluid, that it is homogeneous, and that it is not a mere mixture of serum and fibrine. It may thus be inferred, that *fibrine and serum do not exist as such in the liquor sanguinis, or any other part of the circulating blood.* Dr. Babington goes so far as to say that, although their separation may take place in the body under diseases, it can never do so consistently with healthy action. There is no difference between the fluid described by Müller and Babington, and the plasma of Schultz, except that the first-mentioned physiologist regards it as a chemical solution of fibrine in serum, the second has made his observations more particularly on the transparent part of the living blood after its extravassation from the vessels, and accordingly during the changes which take place while it is losing its vitality, and the last refers to the same fluid viewed microscopically as it circulates in the living body. No purpose but that of confusion would be served by employing two names for one and the same thing. I, therefore, propose to adhere to the designation used by our own countryman, when I refer to this part of the living blood.

There can be very little doubt that the liquor sanguinis is the true nutritive part of the blood, from which all the organs of the body are nourished, since the red corpuscles are never known, as corpuscles, to leave the capillary vessels. Schultz describes it as a tenaceous plastic liquid, and believes it to be formed from the corpuscles, and that the latter are accordingly indirect organs of nutrition. According to this physiologist, also, foreign matters introduced into the circulation, and deposited in the organs, or excreted, are contained in the liquor sanguinis, and do not go to the corpuscles. Thus, indigo gives it a greenish tint, resulting from the mixture of the blue of this substance and the yellow colouring matter of the blood. Rhubarb renders it of a deeper yellow, and its colour is heightened or modified in jaundice. According to the present state of our knowledge, it appears, therefore, that the liquor sanguinis exerts its influence principally in *nutrition* and *secretion*, and the corpuscles on the *nervous, respiratory*, and *muscular* systems, the former being closely related to the organic or vegetative part, and the latter to the animal part of the living system.

When blood is drawn from the vessels of a living animal in a state of health, and allowed to remain at rest, under ordinary circumstances, it undergoes the well-known process of spontaneous coagulation. The halitus is given off immediately, a thin film is formed on its surface, near the edge of the containing vessel, in a very few minutes, and its state of fluidity is rapidly converted into that of a thin jelly. Subsequently its more solid parts concrete, and it separates into two portions—an albuminous liquid, called the serum, and a soft solid, known as the clot or crassamentum, which may either float or sink in the former, and which frequently presents a fibrous or irregular network appearance. The coagulation of human blood is generally effected in from three to eight minutes; the subsequent concretion and effusion of serum take place mainly in from one to three hours; but the process goes on as long as the fibrine retains any degree of vital contractility, which may be several days. The whole phenomena of coagulation and concretion vary greatly in different animals and in different individuals, and they are greatly modified in different states of the constitution, in diseases, and by the influence of various external causes.

Various explanations of the phenomena just described have been advanced at different times. The opinion of Mr. Hewson, that it depends on the contraction of fibrine, is now generally received. One of the strongest proofs of the correctness of this opinion, is the fact, that when this animal principle is removed from blood immediately after its extravasation, the coagulation of the fluid is entirely prevented. The physiological properties of fibrine corroborate this proof.

The chemical qualities of fibrine, as an animal proximate principle, have already been detailed. The physiological properties, in a moist condition, with a great proportion of water, or the elements of this fluid in its composition, are those which concern us at the present moment. It was first described, as it appears, in an isolated state, by Malpighi, and the terms *hepar, insida, gluten, lymph, coagulable lymph, coagulating lymph, coagulum, coaguline*, and *clot*, have been frequently employed by authors to designate, not only the coagulum of blood, but the principle on which its coagulation depends. When deprived of extraneous matter by

washing, we have seen that this substance has the appearance of white strings, laid in striæ, and resembles in form and distribution the planes of muscle which encircle the bladder. It is distinguished from all other animal substances, and especially from albumen, as follows:—1. It always presents a fibrous or filamentous texture; albumen never does. 2. It resists considerable pressure, being firm and elastic; albumen when coagulated is readily broken down between the fingers into a pulpy mass. 3. It becomes solid spontaneously when blood is separated from the living vessels; albumen has no such property. 4. The chemical properties differ slightly, as shown in Table III. and IV., and in the commentaries made upon them.

The following experiments, by Müller, on the formation of fibrine, are extremely interesting. He collected blood from the heart and vessels of a frog into a watch-glass, and observed the commencement of small transparent clots before the whole mass coagulated; he then examined a drop diluted with serum, under the microscope; the corpuscles were widely separated, but the construction of a coagulum which connected them together could be discerned, however wide the intervals between them, and, by placing a needle between any two corpuscles, and moving it about, he could set the whole mass in motion. Müller also adopted the method of filtering frog's blood before it coagulates. The large elliptical corpuscles remain behind on the filter, and the limpid liquor sanguinis passes through. A small glass funnel and common white filtering paper, with the blood of one frog, are sufficient for this experiment. The filter must be moistened, and the blood, when placed on it, should be diluted with a little water. In a few minutes, a coagulum forms in the filtered liquor sanguinis, but, owing to its transparency, it must be drawn out of the fluid with a needle, to be remarked. This coagulum gradually contracts, and becomes whitish and fibrinous, it then acquires, as viewed under the microscope, a finely granulated aspect.

The fibrine, and its property of coagulating, may likewise be observed by means of a beautiful experiment with sugar and water. Cast blood, either arterial or venous, immediately after it is drawn, and before coagulation commences, into water holding about one-sixteenth part of sugar in solution; the coloured corpuscles are by this means precipitated, and the fibrine remains suspended through the whole mass of fluid, forming "a sort of rude cellular structure, by the interlacement of its fibres, representing a rare and loose coagulum. If we imagine these distended cells contracted on the corpuscles, we have the regular clot. On examining the fibrine thus coagulated, under the microscope, a regular conformation is seen, resembling the forms assumed by organised matter, as meshes and ramifications intermingled, anastamosing in a variety of ways." The same phenomenon may be studied by means of water holding a small quantity of salt in solution, which latter substance, like sugar, does not dissolve the corpuscles, but promotes their separation from the liquor sanguinis. The circumstances and experiments detailed, leave no reasonable doubt on the minds of physiologists, that the coagulation of blood after its extravasation from the living vessels, depends on the fibrine and its power of contraction, and that the clot is formed essentially of this coagulating and contracting principle, inclosing within its meshes the blood corpuscles.

A great deal of doubt hangs over the less obvious phenomena of coagulation. I mean as to what actually takes place during its progress, from the period at which the blood is drawn, till the perfect concretion of the clot. We have seen that the fibrine contracts and encloses the blood corpuscles within its meshes, and that the liquor sanguinis resolves itself into serum and fibrine there can be very little doubt. The following questions have arisen:—What other substances are contained in the clot besides fibrine and blood corpuscles? What are the relations of coagulation with respect to caloric? What gases are absorbed or given off?

I. When we examine blood recently coagulated, we find, as originally observed by Boyle and Hewson, that the crassamentum, under ordinary circumstances, bears to the serum the proportion of from one to two-thirds of the whole quantity. On examining the chemical tables, we find only 80 to 130 parts out of a thousand assigned to the clot. This arises from chemists, for their especial purposes, regarding all the water and salts as belonging to the serum. We must be particularly guarded not to admit this as any other than the chemical constitution of the blood. With respect to the salts, there is no proof that some of them are not, in minute quantities, essential constituents of the red corpuscles; and, with regard to the fibrine of the clot, a portion of the water, if not of the salts, undoubtedly belongs to it, but at present the clot is usually regarded as *a mixture of serum, fibrine, and red corpuscles.*

II. Since fluids give out caloric, on becoming solid, it might be inferred, *a priori*, that the coagulation of blood is attended with an evolution of heat; but there are several exceptions in nature to this general law, and innumerable experiments have been made to determine the point as regards the blood, with contradictory results. The affirmative was originally maintained by Fourcroy; in one experiment he found the thermometer rise 11°! Dr. Gordon watched the effect on moving a thermometer, during the formation of the clot, first into the coa-

gulated, and then into the fluid part, by which he detected a difference of 6°, and this remained perceptible for 20 minutes after the process of coagulation had commenced. On repeating the experiment with blood drawn from a person labouring under inflammatory fever, the rise of the thermometer was 12°! Other experiments have been made, by comparing the rate of cooling of the blood with that of other liquids. The result of Sir C. Scudamore's experiments is also on the affirmative side of this question; he found a slight rise of the thermometer at the moment of concretion. Hunter, Davy, M. Edwards, Thackrah, and Raspail, dispute the fact. One thing is certain—if a rise of temperature takes place, anything approaching in degree to that mentioned by Dr. Gordon, it is totally inexplicable on the chemical theory of latent heat, the quantity of fibrine being so small that its solidification could scarcely affect the thermometer in any sensible degree. We can only, then, conclude that *there is great reason to believe, but it is not yet proved, that a quantity of caloric is extricated during the coagulation of fibrine.*

III. We have already seen that carbonic acid, and, perhaps, other gases, may be abstracted from the blood by certain processes of experiment, and it has been asserted that the extrication of the elastic fluid mentioned is essential to coagulation: but, according to Dr. Davy, a slight compressing force, which would not interfere with coagulation, is capable of preventing the separation of any gas whatever. Sir C. Scudamore performed a series of experiments (the results of which will be laid before you in a future lecture) by which it appeared that carbonic acid was given off during coagulation. It is also urged, "that the period of time in which the blood coagulates depends in a great measure on the quick or slow extrication of carbonic acid gas. Its evolution takes place most freely as the blood begins to concrete, and ceases when the process is completed. It is evidently an essential circumstance, as the same causes which retain it delay coagulation." But Sir Charles's experiments were made on diseased blood, and a few by Mr. Thackrah, made upon healthy blood, furnished negative results. Moreover, Dr. Davy has rendered it highly probable that the gases of the blood are greatly modified by diseased states of the system. There can be no doubt that gases are given off with the halitus of the blood, but, as well as I can determine, *it is not proved that the extrication of an elastic fluid is a circumstance essential to coagulation.*

This remarkable phenomenon is of so much importance in theoretical and practical medicine, that I am desirous not to omit any considerations which are calculated to elucidate its true nature. Notwithstanding the strong facts already detailed, much discussion has taken place respecting *the origin of the fibrine*; more especially Home, Prevost, Dumas, and some others, have contended that it is derived from the nuclei of the red corpuscles. But the following facts prove this to be an error:—1st. When blood is defibrinated it becomes uncoagulable, as already described, but the corpuscles remain entire. 2ndly. The nuclei are not soluble in acetic acid after the dissolution or rupture of their envelopes. 3rdly. The lymph and chyle, which contain a considerable quantity of fibrine and very few corpuscles, are spontaneously coagulable. 4thly. The buffy coat, constituted entirely of fibrine, according to the experiments of Lassaigne and Müller, occurs in blood which furnishes an abundance of this principle. These considerations, and the experiments before related, show incontestably that it is derived from the liquor sanguinis. A further question has arisen as to whether fibrine pre-exists, either *suspended* or *dissolved*, in this liquid, or whether it is formed from it by a *chemical* or *vital* process, under particular circumstances? I cannot agree with Denis, Dugés, Berzelius, Gruilhuisen, Burdach, and Wagner, that the above facts prove the existence of the fibrine in a state of solution in the blood; nor can I concur with Schultz and Magendie, that it forms as a chemical product after death. The process in question does not take place in the true serum, it occurs only in the liquor sanguinis before its death. I regard coagulation, and the contraction of the clot, in the light of an organic process, as shown by the coagulated fibrin having an organized texture, already so beautifully described in a quotation from Magendie himself.

Many *causes* have been assigned for the formation and contraction of the fibrin, and the consequent coagulation of the blood. 1, *Rest.* Spallanzani stated that he saw the blood coagulate in an animal in which the circulation was arrested, and liquify anew when this was re-established, but contradictory facts were afterwards brought forward. Hunter gave an instance of the blood remaining fluid sixty-five days in the *tunica vaginalis*, and coagulating after extravasation. Hewson proved that rest alone is not its cause, by tying the jugular vein of a living dog in two places. After a considerable time, although still coagulable, the blood was not coagulated. 2, *Cold.* By freezing blood, and then thawing it, without producing coagulation, Hewson proved that it is no more owing to cold than to rest. Coagulable blood remains liquid in cold-blooded animals, or its circulation could not be maintained. 3, *Air.* Hewson regarded this as the most powerful coagulating cause; but air is not essential to the process, since it frequently takes place in the vessels and cavities of the body after death, and even, as in fibrinous concretions,

during life. Air has been found, after death, in the blood-vessels with fluid blood. Experiment also proves, that the admission of air frequently fails to induce coagulation, and its exclusion to prevent it. Air is, therefore, not the cause of coagulation. 4, *The Escape of Gas.* According to direct experiment, coagulation takes place without the escape of carbonic acid or any other elastic fluid. 5, *Loss of Nervous Influence.* By this, most likely, is to be understood, all the vital properties of which the blood is possessed. Mr. Thackrah says "the vital or nervous influence;" according to this view, coagulation is the death of the blood. But, as remarked by Dr. Elliotson, the blood is often completely dead, and remains fluid; hence it is clear that coagulation cannot be looked on as the death of the blood. 6, *Evaporation of Aqueous Particles.* This is open to the objection urged against carbonic acid gas, and is totally inadequate as a cause. 7, *A Last Effort, or Loss of the Vital Actions of the Corpuscles.* Fibrin is not derived from the corpusles, and coagulation takes place in the liquor sanguinis, completely isolated from these bodies. 8, *Vital Power.* Hunter describes it as a process similar to union by the first intention, or to that of the contraction of muscles after their separation from the living body. He terms it "an inherent power of self-coagulation." If this is not correctly expressed, it undoubtedly points to the true cause of the coagulation of the blood.

Every attempt to explain this phenomenon, on chemical or mechanical principles, has signally failed. I believe I may say, that the numerous experiments and the strenuous efforts which have been made for the purpose, prove it to be *impossible*, and although the employment of the term vital power may appear to be a mere expression of our ignorance, yet if we can convince ourselves of it as a fact, we, in truth, advance our knowledge greatly, by ceasing to compare this remarkable process with mere inorganic agencies and effects, and by studying it analogically and deductively, in connection with vital phenomena.

In the coagulation of the blood we have clearly developed the principle of *contractility.* Dugès compares it to a post-mortem rigidity, so that blood, once coagulated, can no longer be considered as living; and he regards the subsequent contraction and separation as a physico-chemical effect. Dugès describes the comparison made by Aristotle and Hunter, between this process and muscular irritability, as erroneous, since we have no example of the return of the blood to a liquid state after its coagulation. Yet some of the experiments of Spallanzani seem to prove the reality of such an occurrence; and Mr. Cornish, as quoted by John Hunter, found the blood of the bat, in its torpid state, in a certain degree coagulated, but soon recovering its fluidity on motion and increase of temperature. Whatever credit may be due to these statements, we shall find, as we proceed, that the process may be accelerated by the same agents which excite contractions in *irritable* muscular fibre. Small quantities of chloride of sodium, or of ammonia, both powerful stimuli to irritable muscular fibre, for instance, hasten the coagulation of the blood. Muscular fibre does not contract by the effect of stimuli, when kept at a temperature of 32°; nor does blood coagulate; but the irritability of both is evinced when they are again raised in temperature. Muscles, like blood, have a tendency to contract on exposure to the air—their sudden convulsive snatchings, when just dead, are familiar to every one. Not that the two things are identical, for the state of relaxation and repose exhibited by muscular fibre, are in no way evident in the contraction of fibrin. John Hunter seems to have erred, by endeavouring to establish too close an identity between them. When a limb is withdrawn from a living body, it exhibits vital effects for a time, but they are fewer in number than before its separation, for we do not imagine it to retain any degree of animal sensation. Extravasated blood exhibits fewer vital phenomenon than detached muscle. In proportion to the number and vigour of its vital effects, we say, a substance has a high or low degree of vital power. Fibrin, then, has a low degree of vital power. Mr. Prater has directed attention to the existence of a closer analogy between the fibrine of blood and the muscular fibre of the bladder than that of any other muscle in the body. "If an animal just dead, in which this organ is empty, be opened, it is not found contracted on itself, but after exposure to the air it is seen gradually to diminish very much in size; this contraction may be hastened, as in the case of the blood, by more powerful stimuli than air; if the organs be removed from the body, after its contraction is once complete, there is no tendency to relaxation as in other muscles, and in this respect it resembles the contraction of the coagulum of blood." Mr. Lane has made a similar remark to me with respect to the persistance of contraction in the descending colon, which he has observed to remain contracted to one-third of the diameter of the rest of that intestine, till the commencement of decomposition. Thus the irritability of the blood seems to admit of a better comparison with some of the muscles of the body than with others. Another proof of the existence of vital power in the blood, after its extravasation, much insisted upon by Mr. Prater, is the following. Various agents produce widely different effects upon it, before and after its coagulation. Thus, the proper chemical effect of bi-chloride of mercury is to coagulate the albumen of dead blood, but six grains of this salt added to two ounces of sheep's blood, before coagulation, prevent the process altogether. So

also a heat of 140° — 150° applied to blood immediately after it is drawn, keeps it permanently fluid, while the same temperature coagulates, or rather *consolidates*, a mixture of clot and serum, rubbed up together, so as to resemble, as much as possible, the state of the fluid during life. We have, at present, no evidence of any other vital properties in the fibrine but those of *contractility* and *irritability;* but, as that eminent physiologist, Magendie, says, although it must be confessed that he is frequently inconsistent with himself, "It is neither by a chemical action, nor by a process analogous to crystalisation; there is something more than this in the formation of the net-work of the clot; there is something in it which tends to live."

Many things in the history of the blood are explained by this undoubtedly correct view of its coagulation. This process will not take place at all times and under all circumstances; *coagulation will, in fact, take place only so long as blood is living.* If you kill the blood the fibrine will never coagulate, and this is true, whether its vitality be destroyed before its abstraction from the living system, as frequently happens in sudden death, or afterwards. Many substances added to the blood after its extravasation, by destroying its vitality, prevent its coagulation. Many substances injected into the veins of living animals, by destroying its vitality, prevent its coagulation. Many agents in the system, as causes of disease and death, and many powerful therapeutical agents, destroy the vitality of the liquor sanguinis and hence the contractility of the fibrine, or by acting upon its vitality, suspend, hasten, or destroy, the coagulation of the blood.

Before these last-mentioned effects can be studied, we must make ourselves acquainted with those of certain influences to which blood is necessarily subjected, when apart from the living system. The best practitioners have always been aware of the importance which attaches to its coagulation in certain diseases, although the consideration of this quality, as relates to other maladies, in which it is equally important, has been too much overlooked. Dr. Wilson, however, at all times urged this subject most forcibly upon the pupils of this school. Even in diseases, and classes of diseases, upon which volumes have been written during the last half century, without even so much as mentioning the blood at all, Dr. Wilson appreciated this most important consideration. In his own words—"as respects the influence exercised by the very existence of this principle, we know that a range of time and degree exists, for we have registered it in faintings and inflammations. Many sensations in *hysteria* and other diseases may depend on incipient coagulation." So universal is the range, and so powerful is the influence, which the state of coagulability of the blood exercises in the living economy, that it behoves us to acquaint ourselves with every thing which is known on the subject. I have already cautioned you against reasoning by analogy from dead to living blood, but the circumstance that its vitality is retained for some time after its extravasation, enables us to investigate the vital effects of many influences upon it. Here, also, the analogy between the living blood at rest, with its volatile constituents rapidly dispersing, and all its parts in a state of incipient transition from life to death, and the same "ever-moving, ever-changing" fluid of life, so beautifully described by Dr. Wilson, is by no means very close. Yet both are living, and if we can expect to obtain any knowledge of the effects of agents upon the living blood as it flows in its vessels, by observations and experiments made on blood after its abstraction from the system, it must be previous to, or during coagulation, or before the vital principle is extinct. The following are the general influences to which I have alluded:—

I. *Atmospheric Air.* Hewson mixed air with blood in the jugular vein of an animal, and found the coagulation promoted, and he accordingly inferred that it is the most powerful agent in the process. Prater repeated the experiment with the same results. So, also, from the following experiment by Dr. Scudamore, we know that coagulation is retarded when the atmosphere is simply excluded. Two stoppered bottles were filled with blood, the stopper was inserted into one, and left out of the other; after five minutes, that in the closed bottle remained nearly fluid; that in the open bottle was coagulated, the temperature of each being 82°. Air admitted to blood at rest hastens the effect of rest itself. Mr. Thackrah's experiments tend to show, that although the exclusion of air retards the commencement of coagulation, it favours its completion, since, on comparing blood 24 hours after it is drawn, the exudation of serum is much more complete when the atmosphere has been excluded.

It is no doubt to the action of the air that the influence of *extent of surface* is, for the most part, to be referred. When extravasated blood presents a large surface, the period at which coagulation takes place is shortened, but the separation into serum and clot is retarded. This is frequently observed when blood is received into cups standing on a plate; no separation takes place in the portion spilt on the plate, provided the layer be thin. So, also, when blood flows down the arm during venesection, or trickles over a considerable surface, coagulation ensues almost immediately. A fast or slow stream, in venesection, influences the process in the same manner. Sydenham, Huxham, Hey, and Hunter, particularly, observed that inflammatory blood differs in appearance according to the degree of rapidity with which it flows.

When the stream is weak and slow, especially as before said, when it trickles down the arm, it coagulates quickly. When the stream is strong or rapid, coagulation is proportionally retarded. Flowing from the same vein, one portion in a full stream into the receiver, another portion trickling down the arm, several minutes elapse before coagulation begins in the former, whereas, in the latter, it begins immediately. This is particularly observable when the quantity is small. When the surface exposed is considerable, but not so great as to prevent entirely the separation of parts, the natural process is greatly disturbed. Sometimes when the separation of serum from clot is prevented, there is a tendency in the corpuscles to subside from the serum and fibrin, leaving behind a diluted buffy coat. The explanation of all these circumstances, according to the principles already explained, is this:—The atmospheric air acts as a strong stimulus to the vital properties of the liquor sanguinis, so as to promote coagulation under ordinary circumstances, but when it is applied too suddenly, or too extensively, it exhausts or weakens those vital powers, so as to lessen the contractile energy in the latter stages of the process.

II. *The Form and Material of the Vessel.*—Mr. Hey first made the observation that the material of which the vessel is composed, into which living blood is received, influences its coagulation. Mr. Thackrah made experiments on the influence both of the shape and material of the vessel. Blood coagulated sooner in metallic than in glass and glazed earthen vessels, and the quantity of serum separated is smaller in the former. In one specimen of blood divided into five parts, the proportion of serum to clot was as follows:—

Vessel.	Serum.	Clot.
Copper	10 to	18.9
Silver.....	10	22.6
Glass.....	10	24.4
Gallipot.....	10	37.3
Pewter	10	175.

But these results were, in part, influenced by the shape, and accordingly, by the extent of surface. However extraordinary that in the pewter vessel may appear, Mr. Thackrah asserts it to be correct. Upon the whole, according to the experiments by the last-mentioned physiologist, the form and material seemed to produce irregular effects. This has been referred to the electrical condition of the different vessels. Scudamore attempts to account for it upon the principle of a difference in the conducting power of the material, metals being better conductors of caloric than glass and earthenware. However this may be, on examining the results of experiments, I see nothing contradictory to the principle that when the liquor sanguinis is more powerfully acted upon by external stimuli, provided they are not so strong as to destroy vitality altogether, the coagulation of the blood is hastened, but the subsequent contractile power of the fibrine is debilitated or exhausted.

III. *Mass.*—The greater the quantity of blood, the later the coagulation. Hunter thus expresses it: "the contact of blood with blood retards coagulation." This is, in some measure, the reason why a deep mass is longer in coagulating than a shallow one, and has something to do with the quick coagulation which is observed after a slow stream. A curious appearance connected with the mass of coagulating blood, has been noted by De Haen and Hewson. The latter having bled a person into four cups, at 10 o'clock in the morning, on looking at the blood at five in the afternoon, the serum in the first cup was found not separated from the clot, but the clot felt as if it contained a fluid in a bag. Upon pressing it, the fluid gushed out, and, in a few minutes after exposure to the air, it coagulated. The explanation of this phenomenon seems to be, that the fluid in contact with the air and the sides of the vessel, coagulated first, and protected the enclosed liquor sanguinis from external influences; the vitality of the whole mass being sufficient to maintain its fluidity, just as a quantity of blood may be found fluid and still coagulable in the heart of an animal twelve or fourteen hours after death. This subject is too intimately connected with that of vital influences on the coagulation of the blood, to be further pursued in this place.

IV. *Agitation or Rest.*—It is well known that the agitation of blood, immediately after it is drawn, preserves it apparently fluid, and this process has been frequently adopted for culinary purposes. Hey and others, from a superficial examination, inferred, in these cases, that the coagulation of the fibrine was prevented; but Thackrah and Prater performed the following experiment, from the result of which it appears that this fluid appearance is fallacious. Blood, which had been kept fluid by agitation, was afterwards placed in a filter, and the fibrine was left behind in shreds. Strong agitation, therefore, does not prevent coagulation; it merely divides the coagulum, and moderate agitation appears to promote the process. Prater, Davy, and Scudamore, confirm this, although the effect is but slight, and the former explains it by the stimulus of a larger surface of inanimate matter, the last by its favouring the escape of carbonic acid gas.

Exp. Blood received into a gallipot was moderately stirred with a stick for two minutes; it coagulated in three minutes and a half. Another portion of the same stream, treated in the same manner, coagulated in four minutes. Another portion remaining at rest had no appearance of coagulation in six minutes, and at the end of eight was much less firm than the two first portions.

V. *Temperature.*—The coagulation of blood, and its separation into serum and clot, are greatly influenced by the temperature to which it is exposed after extravasation. The process may, in fact, be hastened, retarded, or entirely prevented, by different degrees of heat. These effects will be seen in the following—

TABLE XIV.

Results of Experiments on the influence of Degrees of Temperature, on the Coagulation of the Blood, and the Contraction of the Coagulum; with Authorities.

98°	Favourable to coagulation. This, and a few degrees lower, the standard usually referred to....	Hewson, Hey.
100°—110°	Coagulation is accelerated in blood raised to this temperature immediately after extravasation....	Hewson.
120°—130°	Accelerates very considerably. It also accelerates the contractions of the clot	Hewson, Prater.
140°—150°	Suddenly raised to this degree, coagulability is permanently destroyed. If previously coagulated, it destroys the contractility of the clot	Prater.
165°—170°	Becomes solid almost instantly from the solidification of albumen, but not from the separation of fibrine and its contraction, or coagulation of blood, properly so called	Prater.
65°	Retards coagulation	Scudamore.
53°	Considerably retards.....	Scudamore.
50°—40°	Considerably retards. Human blood kept at 45° remained fluid for full 16 minutes	Hewson, Thackrah.
40°	Remained fluid 20 minutes after being cooled to this degree, and did not coagulate for an hour and ten minutes.....	Scudamore.
38°	In one experiment kept at this temperature for 22½ hours, coagulability permanently destroyed	Hewson.
32°	Kept at this temperature for an hour, remains fluid, but coagulates after removal to a higher. Kept at the same degree for 36 or 40 hours, coagulability permanently destroyed	Hewson, Prater.
30°—20°	Blood freezes. If kept frozen a short time, and then thawed, it coagulates. If kept so for 24 hours, coagulability is permanently destroyed. In blood previously coagulated the contraction goes on at the freezing temperature.....	Thackrah, Prater.

It appears from these results, that a very high or a very low temperature is capable of destroying the vitality of blood, and of preventing altogether the phenomena of coagulation. Hewson had observed that blood drawn from a vein, divided into portions, and exposed to different degrees from 60° to 105°, coagulates in about the same time, and that, at 108° it coagulates and separates sooner than at 55°; and Hey found that cold did not prevent, but suspended the separation of serum, and that a temperature of 45° or 46° greatly interrupted the process. The above results corroborate those of Hewson and Hey. The fact that blood can be frozen without destroying its coagulability, has been adduced as the strongest argument against its vitality, more especially the vital organisation of the fibrine. But the interesting experiments of Mr. Prater completely supersede this objection. The most interesting circumstance of all is, perhaps, the following:—that high degrees of temperature, not sufficient to destroy the vitality of the liquor sanguinis, or of the clot, act as stimuli to vital contractility; on the con-

trary, lower degrees of temperature than ordinary from the abstraction of stimulus, retard both coagulation and contraction. The same degree of heat which destroys the power of coagulating also destroys the power of the coagulum to contract. The vital character of the contraction of the clot is well exemplified by the following experiment from Prater:—"Two equal portions of the same blood were poured into similar vessels; one was immersed in water at 120°, the other into cold water; the former contracted two-eighths of an inch in one hour, the latter only one-eighth; the former being now taken out of the water, was found, when examined at the end of ten hours, to have contracted another eighth of an inch. This further effect proves that the contraction caused by the heat was not the result of any decomposition or disorganization." Although blood is not coagulated, as a fluid it is rendered more dense and viscid by cold.

Air, temperature, motion, or rest, the form and material of the vessel, and the circumstance of mass, are then the known ordinary influences which affect the coagulation of extravasated blood. They must be taken into account in every observation which is made on this process, with a view to elicit information as to the state of the living system. Of these influences, one acts far less powerfully than two or more, so that when we only in part, as it were, place blood in unnatural circumstances, its tendency to coagulate is much less than if we place it more decidedly in such a state. Rest produces a great tendency to coagulate; then comes the admission of air: combined, their individual powers are much increased. These agents, we have seen, are not causes, and they have more or less effect, according to the weakness or strength of the vital power of the fluid upon which they act. Many experiments have been made upon this interesting process; these we shall have to consider, as also the subjects of the period of coagulation, the proportion and density of the clot, and the influence of states of the living system, at our next meeting. You will be fully apprised that none of these circumstances could be correctly estimated, without due attention to those which have already been discussed.

John Hunter remarks, "Life could not go on without the property of coagulating in the blood; for, as all the *solid* parts are formed from the blood, this could not take place if there did not exist such a power of coagulation;" and Dr. Wilson has stated in this theatre, that "the tendency of blood to coagulate without actual coagulation, is no doubt modified, if not suspended, by the impressions constituting disease, whether received from the air, from the food, or from poisons. Some medicines may produce their effects on the system, by influencing the same principle of coagulability in the blood; for medicines act by the same means as air, food, poisons, and other impressions." Thus, gentlemen, we are not only lineal descendants, but virtual disciples of the School of the HUNTERS.

COURSE OF LECTURES
ON THE
DISEASES OF THE EYE,
Delivered in 1839-40,
AT THE
ROYAL SCHOOL OF ANATOMY AND MEDICINE,
MANCHESTER,
By JOHN WALKER, Esq., Surgeon.

LECTURE III.

CONJUNCTIVITIS PURIFORMIS; OR, PURULENT OPHTHALMIA.

THE next variety of conjunctival inflammation I shall treat of is, *conjunctivitis puriformis*; *or, purulent ophthalmia*, so named from the appearance of the discharge poured out from the conjunctival surfaces. This variety is also frequently termed Egyptian ophthalmia, and sometimes contagious ophthalmia. The former of these appellations has been assigned to it on account of its presumed origin; the latter, from the mode in which it is supposed to be communicated.

Of conjunctival inflammation, there is no form so severe or dangerous as that to which I now invite your attention. Conjunctival inflammation in this form is, indeed, one of the most important of all the affections of the eye; and it is even probable that a greater number of individuals are deprived of vision from this disease than from any other cause. Hence purulent ophthalmia has justly excited, particularly of late years, the earnest attention of surgical practitioners; and to no class of the profession are we more indebted for information concerning its nature, and the means of treating it, than to the intelligent and highly respectable class attached to the military service of the country.

There are many affections, as well of the eye as of other organs, which may be safely trusted to the unaided efforts of nature. Purulent ophthalmia, however, is not one of them; for if there be a disease which more than another requires the vigilant attention of the ophthalmic surgeon, it is this, as will, I think, be fully proved by what I intend to lay before you.

In presenting my remarks on purulent ophthalmia, I shall imitate the example set by authors, in general, of first treating of the ordinary purulent ophthalmia of adults; next of gonorrhœal ophthalmia; and, lastly, of the purulent ophthalmia of infants; and I shall do so, not because there is any material difference in the nature or treatment of

these several varieties, but because there are some peculiarities connected with them, which, by adhering to this arrangement, may be more appropriately pointed out.

The *purulent ophthalmia of adults*, then, is the subject first to be discussed.

The symptoms which indicate its existence are similar to those of catarrhal ophthalmia, though of a much more intense and severe description. The two affections have been regarded, by some writers, as modifications of the same disease; and, on a first view indeed, the distinctions between a very severe case of catarrhal and a mild case of purulent ophthalmia are not very strongly marked.

The most striking symptom of this variety of ophthalmia, and the one which serves to characterise it, and to which, as I before observed, it owes its ordinary appellation, is a profuse discharge of a thick, yellowish fluid, resembling pus. This fluid, however, is not really of a puriform nature; it is merely the mucous secretion of the conjunctiva changed in character, and is more correctly termed muco-purulent. It is secreted in very great quantity, and from every part of the conjunctiva; but more particularly from its palpebral portions; and if it does not escape freely, it lodges within its folds, and thus, perhaps, contributes in some degree to the distention of the eyelids.

The next symptom, which is especially remarkable, is the enormous swelling of the palpebræ, especially the superior. These are so much swollen from infiltration into their cellular tissue, as to render all voluntary attempts at separating them perfectly fruitless; they are also, externally, almost invariably of a deep red colour. In general, however, the palpebræ are easily separable by the judicious efforts of the surgeon, although in the early stage there is sometimes so much tension as to render it difficult.

When this separation has been accomplished, and the matter carefully removed, we then perceive that the conjunctiva is extremely red and vascular, in short in a state of chemosis,—a state which is devoloped in this disease to its fullest extent; the conjunctiva being considerably raised, an œdematous effusion having been poured into the cellular texture under it, and a considerable part of the cornea, all round its circumference, being buried under the overlapping folds of the distended membrane. Sometimes the swelling of the conjunctiva is so great that it even protrudes between the margins of the lids.

The consequence of the conditions just described is, that generally the patient has great pain about the eye and adjacent parts, which disturbs his rest and occasions him much anxiety. The constitutional irritation is otherwise not considerable, there being seldom any febrile symptoms developed. There is generally some intolerance of light when the eye is exposed, but this never happens except for the purpose of examination or the application of remedies, owing to the excessive enlargement of the eyelids.

The symptoms now mentioned are not, strictly speaking, those which present themselves in the first stage of the disease. The first or purely inflammatory stage seldom continues more than about 30 hours, and is but rarely witnessed by the surgeon, as the patient scarcely ever applies for assistance until that time has elapsed. But when it does come under notice, nothing is perceived essentially different from what occurs in a severe attack of ordinary conjunctivitis, the chemosis and the purulent discharge not being then fully formed.

Purulent ophthalmia usually attacks both eyes, and it is said more commonly begins in the right. It is certain that the one first attacked is generally the worst; that which is last affected frequently has the disease in a mitigated form. In most cases there is an interval of one or two days between one eye becoming affected and the disease appearing in the other.

Distressing and unpleasant as this condition of the eye must certainly be to the sufferer, if the disease be confined to the eyelids and conjunctiva, no danger can result to vision; but, unfortunately, it very frequently happens that the morbid action extends to the cornea, and proves destructive to vision. There is no perceptible vascularity of the cornea, at least in the early stages of the disease; and although there is undoubtedly inflammation of that texture, yet its existence is not manifested, except by its consequences, ulceration and opacity. The inflammation of the cornea is of the same violent character as that exhibited by the conjunctiva, but the difference in structure of the two parts occasions a considerable difference in the phenomena which result from this process. The structure of the cornea is so peculiar, that when inflamed it is exceedingly prone to fall into the condition of ulceration, and when the inflammation is more intense, it very frequently even sloughs. In the case of ulceration, the ulcerative process is not always confined to its conjunctival covering. Deep and extensive ulcerous excavations are apt to occur, particularly around the circumference, and to penetrate the various laminæ of the cornea; and, in the worst cases, it is not uncommon for the whole or nearly the whole of the cornea, sooner or later, to slough, for the humours to be evacuated, and the eye destroyed. In cases in which the ulceration, though it penetrates the cornea, is more limited in extent, a portion or portions of the iris protrude, and sometimes the crystalline lens escapes. When this last event happens, viz., escape of the crystalline lens, vision is seldom restored. With respect to opacity of the cornea, this may occur to a greater

or less extent, either with or without ulceration.

Much has been recently said by various authors, as by Dr. Mackenzie, Mr. Tyrrel, and others, respecting the cause of the sloughing condition of the cornea, of which I have just spoken. Mr. Tyrrel, more particularly, has insisted that it is to be attributed to pressure produced by the distended chemotic conjunctiva preventing the passage of the blood to the cornea, and thus giving rise to mortification and sloughing of it. I must candidly confess I cannot imagine that a membrane of so light and spongy a texture as the conjunctiva, even in a state of chemosis, can possibly produce such an amount of pressure as to cause strangulation of the vessels proceeding to the cornea. Of one thing I am certain—I have seen chemosis in its every grade of intensity which has not been succeeded by any sloughing; and, on the other hand, I have frequently seen sloughing without any preceding or accompanying chemosis.

Although purulent ophthalmia may be produced by the direct application of its own morbid secretion to a healthy eye, yet I conceive it to be much more frequently originated independently of such application. Thus, I have seen many instances of this disease in individuals who had not been near any one similarly affected, and in the vicinity of whose residences no other person was suffering from it. In these instances, it seems to be but fair to conjecture that the affection has resulted from cold or some of the ordinary agents which induce ophthalmia, operating upon individuals of peculiar predispositions.

Cases do now and then however occur, in which the disease has to all appearance been communicated from affected individuals to others who have been in attendance upon them. I recollect an instance of this sort, which occurred some years since at the ophthalmic hospital of this town. An infant was attacked with purulent ophthalmia, which was of so severe a character as to destroy both eyes. An elderly female who had the care of the child also became similarly affected, from, as she stated, having accidentally had some of the purulent matter brought in contact with her own eyes. The disease in this female was of the same virulent character as that in the infant, and was also destructive of both eyes.

Purulent ophthalmia prevails very extensively in eastern countries; in Persia, India, and more particularly in Egypt; and in those countries it is probably produced by the minute particles of sand with which the atmosphere is so often loaded, and which are brought into contact with the eyes; and perhaps also by the powerful reflection of the sun's rays from the sandy and desert wastes. The Egyptians are said not to regard it as being at all contagious.

The opinion has generally prevailed that purulent ophthalmia was originally introduced into Europe from Egypt, by the British and French troops, when they returned thence about the year 1802. Great numbers of soldiers were infected with the disease whilst residing there, and many of them were still labouring under it when they arrived at home. It is certain that the attention of English surgeons was not properly directed to it until after the Egyptian expedition; but that cases of purulent ophthalmia must have occasionally appeared in this country prior to the epoch just mentioned, can hardly admit of dispute, inasmuch as we now find them occurring sporadically, and to all appearance independently of any contagious influence whatever.

Purulent ophthalmia, nevertheless, once produced is, in my opinion,

Unquestionably contagious.

I have mentioned one instance which came under my own observation, that forbids me to entertain any doubt upon this point; and Sir James M'Gregor has reported several cases which I deem equally conclusive in reference to the contagious character of the disease, the details of one of which I will give you in his own words:—"A nurse of the Military Asylum Hospital, at about eight o'clock A.M., while washing with warm water the eyes of a boy suffering severely from purulent ophthalmia, inadvertently applied the sponge which she had used to her right eye. She immediately mentioned this circumstance to the other nurses, but took no means to prevent infection. Between 3 and 4 P.M. of the same day, great itching of the right eye took place, and before she went to bed it was considerably inflamed. Next morning her eyelids were swollen, she complained of pain on moving them, and the whole anterior surface of the eyeball was much inflamed. A purulent discharge also began to trickle down the cheeks from the inner canthus. The symptoms increased in severity, and, notwithstanding the means that were used for her relief, the eyeball burst in front of the pupil, on the 4th day after the application of the purulent matter. The sight of the eye was irrecoverably lost, and the inflammation continued for upwards of three months.

But, although I have expressed my opinion that purulent ophthalmia once produced is unquestionably contagious, and although I have mentioned and referred to cases which seem to me fully to prove the correctness of such an opinion, yet I must call your attention to the circumstance, that in the cases brought under notice, there was a direct application of the purulent matter from an affected eye to a healthy one. I am not aware that there is any good ground for thinking that the matter furnished by an eye affected with purulent ophthalmia evaporating, mixing with the atmosphere, and

thus being brought into contact with a healthy eye, can give rise to the disease. Certainly I may mention one fact which, at first sight, may possibly induce you to think otherwise, though when you consider it in conjunction with others that I shall mention, I conceive it likely your opinion will coincide with my own.

A slave ship, Le Rôdeur, in the year 1819, had left the African coast 15 days before any one on board exhibited symptoms of purulent ophthalmia, and there had been no cases of it at the port which she had last left. About 160 negroes were crowded together in the vessel, and each of these was successively attacked. Such was the miserable condition to which they were reduced, that, on bringing them upon deck for the benefit of the fresh air, many of them threw themselves into the sea, in order to put an end to their sufferings. In a short time afterwards the crew were also attacked, one only of their number escaping, and he also became affected after his arrival in the West Indies. So virulent was the disease that 39 of the negroes were blinded of both eyes, 12 lost one eye, and 14 others had vision more or less impaired from opacities of the cornea; and of the crew, 12 men, one of these being the surgeon, lost both eyes; five, one of these being the captain, lost one eye, and four others had extensive opacity of the cornea with adhesion of the iris.

Independently of the question of contagion, I may observe that the history of this slaver affords a melancholy and instructive illustration of the frightful nature of this affection. Upwards of 50 persons, within the few weeks necessary to cross the Atlantic, all of them previously healthy, were attacked with a disease which, without materially affecting their bodily condition, left them in a state of total blindness, and nearly 40 more had each one eye either entirely destroyed or seriously injured!

Now, at first sight, as I have just hinted, it may appear to you that, in this example, the disease was propagated from one individual to another by contagion. Such a conclusion to myself seems not to be correct. I cannot suppose that in every instance in which the disease originated, there had been a direct transfer of purulent matter from the eyes of an affected individual to those of a healthy one, as happened with respect to the nurses in the two foregoing cases. And further, I have already intimated that purulent ophthalmia did not make its appearance until the vessel had been 15 days out of harbour, and at the time of its departure did not exist among the inhabitants of the place. To my mind, the disease was clearly of an epidemic character. I think there was something peculiar in the state of the atmosphere in that region and at that particular period, to which the affection ought to be attributed, and, more especially do I think so, because the vessel in question, while on her voyage, encountered another vessel, the crew and passengers of which were in precisely the same condition as themselves. Another singular circumstance, tending to confirm me in my opinion is, that so soon as the crew and negroes of the first mentioned vessel landed, the affection subsided and was not communicated to other persons on shore. And again, it is well known that the disease is often epidemical among soldiers in barracks, and sometimes among children in schools; but let the inmates be removed and they get quit of the disease, without communicating it to others. Moreover, we find that in most of the cases which have been clearly traced to the actual communication of purulent matter to a healthy eye, the disease has been confined to the organ so infected; whereas, in ordinary cases, where such communication has not been established, it is notorious that both eyes are almost invariably attacked. Hence, I conclude that, although this disease is of a contagious nature, yet it cannot fairly be considered as ordinarily resulting from the application of a contagious matter.

Having now given a sufficiently detailed account of its symptoms and probable causes, I proceed to speak of the

Treatment of Purulent Ophthalmia.

The examples presented by the slave-ship amply prove the correctness of a statement previously made, that purulent ophthalmia ought not, on any account, to be allowed to pursue an uninterrupted course, but rather that a most decided treatment should be adopted for the purpose of arresting it. I cannot but think that, with respect to these cases, the disease was left to pursue its course uncontrouled, for the remedies employed, viz., poultices of rice and vermicelli, infusions of elder flowers, blisters to the nape of the neck, and pediluvia containing mustard, I regard as being utterly ineffectual to controul a disease of so severe, so rapid, and so destructive a character. It appears to me that the sailors manifested much more judgment than the surgeon, when they, as we are informed they did, dropped brandy into the eyes of the sufferers, and thus materially benefited them, and even a poor negress exercised more judgment than he, when, upon their arrival in the West Indies, she advised them to drop the juice of lemons upon the eyes, which remedy was also found of considerable use.

I regret that I cannot state with truth, that melancholy results, similar to those which occurred in the examples just brought under your notice, from an inefficient treatment of purulent ophthalmia, are no longer met with; for, in fact, notwithstanding the more extensive knowledge of the nature and treatment of this disease, which now, undoubtedly, prevails among the profession

many cases equally lamentable are continually occurring, in consequence of the treatment had recourse to having been either not judicious or not sufficiently prompt and decisive.

I have just said that the treatment of purulent ophthalmia ought to be prompt and decisive. Unhappily, however, a wide difference of opinion exists as to the means to be employed.

By one class of writers and practitioners, the treatment is conducted on the most rigorous antiphlogistic plan, namely, by large and repeated bleedings, followed by cupping, leeching, scarifying, blistering, purging, and the administration of emetic tartar, in such quantity and so repeated as to keep up a state of continual nausea, perspiration, and faintness. In fact, they treat the patient as if he were labouring under acute inflammation of some vital organ.

By another class, local treatment is regarded as of paramount importance; and they propose to remedy the morbid condition of the eye by the application of very powerful stimulants to the diseased surface. The applications that have been employed for this purpose are numerous, though all are used on the same principle. Thus, some use the strong liquor plumbi subacetatis; others, oleum terebinthinæ, vinum opii, and various strong solutions of oxymuriate of mercury, sulphate of copper, and nitrate of silver. The advocates of the stimulant practice treat the subject of purulent ophthalmia as they would do one labouring under an attack of erysipelas, or of an inflammatory condition of a mucous membrane, viz, by local stimulants and astringents. Indeed, both Walther and Müller, advocates though they be of the antiphlogistic treatment, regard the affection as essentially of a local character, confined in its origin to the palpebral conjunctiva, and the former expressly states that it is "not so much an inflammation as an exanthema."

With these two opposite modes of treatment before us, it may be considered a matter of some difficulty to decide as to which we are to select. Unquestionably, the result of a particular mode of treatment affords the best criterion of its utility, or otherwise. As many talented surgeons have adopted each of the plans of treatment specified, it may be advisable to give a somewhat fuller account of their proceedings, and the effects stated to have been produced by them; by so doing we shall be better able to appreciate the value of each.

Calling to mind that it is only within the last forty years that the subject of purulent ophthalmia has been distinctly before the profession, we can feel no surprise that the treatment of it should still be a matter of dispute.

From the melancholy effects produced by this disease, viz., destruction of the eyeball and extinction of vision, it is not wonderful that surgeons should have entertained an exaggerated and erroneous notion respecting its nature and the tissues implicated. The incorrectness of medical language, too, has a decided tendency to originate and perpetuate incorrect views of the nature, and consequently of the treatment, of disease; and thus we find it to have operated as regards purulent ophthalmia. For example, at one time it was the custom to denominate this affection *suppurative ophthalmia*, a name which would almost of necessity create in the mind the idea of deep-seated inflammation and abscess, which, as the natural inference, it would be concluded must be combatted by the most powerful antiphlogistic remedies. Indeed, it may be doubted if the term ordinarily employed, and which I have adopted as being on the whole, perhaps, as little objectionable as any, viz., purulent ophthalmia, may not be similarly injurious. A more incorrect notion than this of suppuration it would be impossible to promulgate; for, as I have before explained, properly, the affection can be considered only as a purely local inflammation of the conjunctival membrane, which sometimes extends to the cornea, and proves fatal to vision, simply by inducing ulceration and opacity of this latter texture.

Among the many experienced surgeons who have strongly advocated and thoroughly carried out the antiphlogistic treatment, I may mention more particularly Rust, Müller, and Walther, in Germany; and Veitch, Travers, and Lawrence, at home. These gentlemen advocate the abstraction of blood from the arm in quantities varying from 30 to 60 ounces, according to the severity of the disease, the strength of constitution of the patient, and the ease or difficulty with which faintness is produced. Carried to that extent, they report bloodletting to have been decidedly useful, whereas small and repeated bleedings they report to have been of little service.

After free venesection, Walther recommends that a large portion of the inflamed conjunctiva should be excised, the bleeding resulting from which operation he regards as a decided benefit, and he states that the cicatrix left in consequence is a mere line, and of no importance.

Scarpa advises "the circular excision of the projecting portion of the conjunctiva with the curved scissors, at the part where the cornea and sclerotica unite." The same proceeding has been recommended by M. Sanson. Mr. Tyrrel urges the division of the conjunctival vessels, by incisions radiating from the margin of the cornea, so as to relieve the pressure which he supposes to result from their turgid state. I have seen this last practice adopted, but am unable to satisfy myself of its being at all capable of arresting the disease, or that it is deserving of the slightest confidence as a remedial

agent. The former operation I have not seen practised.

The adoption of the depleting system, combined with the exhibition of nauseating medicines, is forcibly urged by Mr. Travers. That gentleman says, "With large bloodlettings repeated, subject to the discretion of the practitioner, until the inflammation yields, a brisk catharsis should be combined, and this followed by a teaspoonful of a solution of emetic tartar every hour, so as to keep up a state of nausea, perspiration, and faintness." He then proceeds to describe the favourable appearances observable, when the violence of the disease has been moderated by this treatment. "But if (he continues) when the lowering practice has been pushed to the extent of arresting acute inflammation, the patient being at the same time sunk and exhausted, the cornea shows a lack-lustre and raggedness of its whole surface, as if shrunk by immersion in an acid, or a grey patch in the centre, or a line encircling or half encircling its base, assuming a similar appearance, the portion so marked out will infallibly be detached by a rapid slough, unless by a successful rally of the patient's powers we can set up the adhesive action, so as to preserve in situ that which may remain transparent." So that, from Mr. Travers's statement, it appears, notwithstanding this very active treatment, the eye will be very frequently lost, except the patient can be speedily extricated from the sunk and exhausted condition to which he has been reduced by the means intended for its preservation.

Now, all these directions may appear to some to be exceedingly plausible. We see very acute inflammation both on the surface of the eye and the eyelids. The inference seems natural enough that we must bleed very largely, in order to reduce that inflammation. Nothing, however, can be more fallacious than the reasonings of these authors, when it comes to be tested by matters of fact. Bleeding, I am satisfied, is not the right, certainly not the best, mode to set about the reduction of the inflammatory state of the eye in the disease now under consideration. I had frequent opportunities, some few years ago, of witnessing this kind of practice carried to its full extent. The patients were bled freely from the arm; they were then cupped and leeched, and had tartar emetic and purgatives, and all the usual remedies of this class freely exhibited. But, in spite of all, the inflammation was not subdued; it proceeded on its course; it extended to the cornea; it was followed by opacity, and ulceration, and sloughing, and the eye was generally lost.

So far, indeed, from this practice being useful, I am of opinion that it is decidedly injurious, inasmuch as it tends seriously to impair the constitution of the sufferer, rendering it less able to struggle successfully with the after stages of the disease, or to repair the mischief when sloughing has occurred.

The subjects of purulent ophthalmia are not, indeed, generally of that class for whom we would willingly order very copious venesection: such, at least, is the conviction on my mind, resulting from observations made principally among hospital patients. The experience of other surgeons upon this point may not coincide entirely with my own, and perhaps military surgeons in particular may have ordinarily a more robust class of patients to deal with. In the cases, too, which have come under my notice, there has never been any marked constitutional disturbance, there have never been any febrile symptoms present, and very often there was rather a depressed than an excited state of the general circulation.

Nor does purulent ophthalmia ever manifest a tendency to extend to the internal textures, so as to justify the employment of the severe antiphlogistic measures which have been recommended. In no instance that I have witnessed has this ever happened; and it is a generally admitted fact, that neither pus nor effused lymph has ever been observed in the anterior chamber.

The indifferent success following the adoption of the antiphlogistic treatment, even by its most ardent enlogists, is very striking. Thus, Dr. Veitch, who was in the habit of taking 50 or 60 ounces of blood from patients affected with this disease, reports that in 50 cases out of 636 both eyes were lost, and that in 40 more one was lost. No doubt, too, in a great number of the rest, vision was much impaired from opacities. Other accounts are still more unfavourable. Surely, if no better success attended the employment of this treatment in other inflammatory affections, practitioners would soon cease to place dependence upon it. And when it is borne in mind that these statements refer to a class of patients who, it is certain, enjoyed every possible advantage, having been placed under the strict surveillance of a military hospital, and generally attended to in the very first stage of the affection, our confidence in it is not likely to be increased.

Another striking fact may be mentioned, which is by no means calculated to add to our confidence in it, viz., that at one period (1810) there were 2,137 individuals in the military hospitals of Chelsea and Kilmainham, who had lost both eyes from purulent ophthalmia, exclusive of others who had lost one eye, and were otherwise damaged, most of whom, I believe, had been subjected to the antiphlogistic treatment.

Most authors, indeed, who have advocated the lowering treatment, are compelled to admit that, in spite of its being most energetically employed, the purulent form of

ophthalmia will frequently terminate in destruction of the organ attacked.

In my next, I shall detail the results stated to have followed the use of stimulants, and draw such practical conclusions as my own experience leads me to consider correct.

NOTICES OF MONSTROSITIES.

By ROBERT FOX, Esq., M. R. C. S. L.,
Godmanchester.

Case I.—Mrs. C., 32 years of age, was delivered, at Godmanchester, Huntingdonshire, December, 1823, of the curious *lusus naturæ* about to be describe[1]. The motion of the fœtus was felt about three hours before birth, which was effected by natural pains, and it was still-born. A mis-shapen head, having two faces, one anterior, the other posterior, and each a striking resemblance of the other. In the centre of each forehead was a single eye, in a circular orbit, without palpebræ; a fleshy projection in lieu of a nose, about two inches long, pendulous, like the proboscis of an elephant; a mere sphincter orifice for a mouth, without labia; no traceable mental process, but the head shelving off by a short thick neck from the cheeks to the shoulders; four arms; the spine dividing at the acetabulum, but forming only one body as far as the umbilicus, which then bifurcated into two distinct well-formed male children; these had their foreparts opposite to each other, as if twisted in the progress of their developement, from the position we might have expected to have found them in, from that of their faces, viz., with backs to each other; a single funis.

This was the patient's ninth labour; she has borne twelve children, all of which, with the exception of the above-described, were well formed. I had no means of ascertaining the peculiarities of organization, or arrangement of the viscera, the parents refusing an examination without a previous money bargain, which I declined.

Case 2.—S. C., of Offord Darcy, Hunts, aged 44, was delivered of a twin female child, Oct. 17, 1839. She had been married twenty years, and borne twelve children previous to the present birth. In this twin the union was from the clavicles to the umbilicus, and it had but one funis. The case was attended by Mr. Woolley, of Buckden, and the preparation is now in my collection. It presents one common cavity of the chest, formed by the junction of the clavicle and ribs with each other, before and behind. It contained one heart, situated in the centre of the chest, immediately under what ought to have been the sternum; one set of lungs, two lobes on the right side and one on the left; one liver; one gall-bladder; and one spleen. The other organs all double, and naturally arranged. Her pregnancy was not attended by any uncommon circumstance; the children were of the usual size of twins, but the left child was a little larger than the right. The labour, though protracted, was effected by natural pains, the left head being born about three hours before complete expulsion took place. The twin was still-born, and the lungs had not been inflated when examined thirty-six hours after birth; the weight of the twin was nearly seven pounds.

We have, of late years, heard much of the Siamese twins, and they presented a singular phenomenon for the observation of the pathologist. Their connection with each other was by a ligamentous band above the umbilicus, covered by the common integuments; and, though they thus formed an united body, they possessed, perhaps, an independent existence, having vital organs, circulation, &c., distinct from each other. The Godmanchester and Offord cases were more remarkable. Possibly in both, but in the latter, we have unquestionably two human beings with some of the vital organs in common, and others, perfectly natural in their form and situation, peculiar to each. There were two brains, two series of digestive organs, with the exception of the liver, gall-bladder, and spleen, which were in common, and two uterine and urinary systems. Yet we have but one heart for the circulation of the blood in both bodies, and one set of lungs, and these preternaturally arranged. To theorise upon their separate existence, and, had they lived, moral responsibilities, would be but to wander in the mazes of romance; for, notwithstanding the speculations of the ingenious, the labyrinths of life and generation have not yet been satisfactorily explored. Ages have passed since man began to speculate upon the subject, and notwithstanding system upon system has been piled, many much in accordance with and others diametrically opposed to each other, each and all professing to unravel the hidden mystery, they have been satisfactory alone to the minds of their founders; and, with all our sophistry, we must yet own "that there are many things in heaven and in earth, that are not dreamt of in our philosophy."

That organisation which is most common to mankind, and in which there is the greatest facility for, and most perfect discharge of its separate and combined functions, is ordinarily called the natural standard, and greater or less instances of departure from this standard, varieties of it, are monstrous. The different races of men on the habitable globe possess peculiarities influenced by climate, habits, &c., which are preserved to their nations and transmitted to their posterities, if no admixture of tribes takes place.

Thus are formed varieties in the human race.

But productions are sometimes given birth to, having no claim to such classification, as beings with two heads and one or more bodies preternaturally joined—or with one head and one or more bodies—or with more or less than the usual number of limbs, and these also, in many instances, imperfectly formed. This class of beings is usually called monstrous. The instances which I have cited of this latter class, in the commencement of this paper, stand not alone; for too many cases, somewhat analagous to them, are upon record, and other variations from the natural standard are to be found within the experience of most medical men. In the *Philosophical Transactions*, from the year 1700 to 1720, abridged, published 1749, 4to, at p. 309, vol. 5, two very similar cases are related. In the first, both were females, and still-born. In the second they were of opposite sexes, and "born alive, but lived but a little while." In vol. 7 of the same work, page 688, is an account of a monstrous double birth, by Mr. Ferry, which lived two months, presenting characters so singular, that I shall transcribe it.

"On the last of December, 1772, I went to Domp Remy la Pucelle, in Lorrain, to see there Sebastiana Camus, aged 44 years, delivered on the 24th of the said month, about eight o'clock in the evening, of two children, or twins, joined together. There is one head, neck, breast, abdomen, and two hands on one side; and there are as many parts on the other; the whole being well proportioned and plump, joining together by the belly, which is common to both, so that one of the heads is in the place where the other's feet should be, and the other head is in its natural place; they have but two legs for both of them, which seem to arise from the transverse apophyses of the vertebræ of the loins on one side; and from the opposite regions of the loins comes out a leg, ending with a joint which bends forwards, and at the extremity forms a small stump, like a finger, articulated by ginglymus. There is but one fundament for both, by which they void their excrements; they have but one naval string, and the parts proper to the female sex are also single. They eat and drink with their mouths severally, and while the breast is given to one, the other cries for it. They sleep and are awake sometimes both at the same time, sometimes separately. Each of these children has been baptised."

In the 10th vol. of the same work, page 1209, is an elaborate "account of a preternatural conjunction of two female children, by James Parsons, M. D., read before the Royal Society, Nov. 17th, 1748," from which the following sketch is taken. The skin of part of the breasts and belly was continued to each child, from the lower part of the sternum down to the insertion of a single funis umbilicalis, and the conjunction of the musculi recti, thus formed but one common abdominal cavity up to the diaphragms of the children, above which each had its own proper thorax, containing separate hearts and lungs. Each child had its œsophagus, stomach, and pylorus, from which a duodenum descended about three inches, and then the two uniting, formed one common ductor jejunum, from the lower part of which proceeded separate cœca, with their appendicu'a, to which succeeded colons, &c., to their usual terminations. In this case the communication formed but one abdominal cavity, which contained "in appearance only one liver of a considerable size and an irregular form, but this consisted of two, in reality, preternaturally joined, as there were two gall-bladders." The subject having been injected, the vascular system was minutely examined, and the chief peculiarity with regard to it, was an artery which arises from the aorta, about the place of the celiac of one child, running along before the liver, and is inserted in the same place in the other. It bestowed branches on the stomach, mesentery, and mesocolons, being about five inches long, for there were neither celiac nor mesenteric arteries in either child.

In the 5th vol. of the *Medico-Chirurgical Transactions* is a compendious and interesting paper upon the subject of monstrosities, by Mr. Lawrence, and numerous cases recited of united twins, and variations from nature, with much speculation upon the subject of them. In vol. 1 of THE LANCET, 1830-31, at page 68, is a case reported at the sitting of the Académie Royal de Médécine, on the 2nd of March, by M. Olivier, in which the eyes were closed and flattened, and edges adherent; they were divided, the eyes found wanting, and their places supplied by adipose substances and reddish fibres. It is there also stated, that on the 30th of March, two similar cases were related to the Académie by M. Luzardi, in one of which both eyes were wanting, and in the other the left was wanting, and the right small, atrophic, and affected with membranous cataract. In the same vol. may be found the case of Geoffrey St. Hilaire's human quadruped. Numerous cases of acephalated fœtuses are to be found freely reported and theorized upon in the *Memoires de l'Acadêmie de Chirurgie*, and also of *L'Encèphalocele*, or Hernie du cerveau, and of instances of double bladders. In the 5th vol. of the *Medico Chirurgical Transactions*, before alluded to, is a paper, describing the practice adopted by Sir A. Cooper, in cases of spina bifida, that of puncturing the enveloping membrane with a needle, and then supporting the parts with a truss. Notwithstanding such variety and valuable information may be extracted from these two other sources, perhaps the readers

of your valuable record will not object to my registering upon its pages some few others of the cases of monstrosity, which have occurred in my practice, or under my immediate observation.

CASE 3.—Mrs. H., of Great Puxton, Hunts, was, in Feb., 1834, delivered of a fine female child, well proportioned in every respect, except in this particular—the knee and foot of the left leg were reversed in position, from the middle of the thigh, and, consequently, the leg bent forwards, throwing the heel to the belly. It lived about a week.

Mrs. S., of Upton, Hunts, was delivered, in Nov., 1819, of a still-born female child, acephalated with spina bifida.

Mrs. B——d, Huntingdon, was confined at the usual period of gestation, 9th of Oct., 1839, with a male child, with hare-lip, and so wide a fissure in the palate as to induce belief that it had no roof to its mouth. From one side of the palate grew a cartilaginous projection, terminating in a knob, which filled up the fissure in the lip. The eyes were closed, depressed, and the eyelids adherent. On each hand was an imperfectly formed supernumerary finger, and the genitals were misshapen. It lived five days.

In all the above cases, the women had other children, well formed and healthy.

I shall close this communication with the notice of a case still living in Sawtry-street, Judith, Hunts.

Case 4.—Frank Sechey appears about 16 years of age, and is in good health, not idiotic, but of weak intellectual powers, having imperfect memory, and crafty rather than sound judgment. The head is badly formed; the ureters terminate in a caruncle, or spongy granulated substance at the navel, which is drawn down to the pubis, on which short bristly hair is beginning to appear; a misshapen scrotum now contains the testes, which descended some time after birth; the penis retracted; glans penis imperfectly formed, with no urethral passage; he has, apparently, a double femoral hernia, from which he derives but little inconvenience, though, from the extreme tenderness of the parts, he cannot bear any attempt at reduction, and halts in his walk. He is afflicted with constant stilicidium.

CONSTITUTIONAL LUES OF LONG EXISTENCE.

To the Editor of THE LANCET.

SIR:—The following case is, I think, one worthy of notice:—

Mr. B., a gentleman moving in the middle walks of life, was affected some years ago with severe pains in the chest and shoulders, and occasionally in the loins. These seldom lasted above three or four days at a time, and were most severe in cold and wet seasons. Three years back, at the commencement of winter, the pains, after attacking his chest and shoulders as usual, descended to his limbs, and affected particularly his groins and thighs in the first instance, but in the course of a few weeks a slight pain was felt over the left shin, which, on examination, presented a slightly ridged appearance, with acute pain on pressure. Mr. B. was at this time under the care of a medical friend, who viewed the case simply as a rheumatic one. I also had at that time occasional opportunities, from my intimacy both with Mr. B. and his medical attendant, of seeing the phenomena of the case, and on the affection of the shin being brought under my notice, I stated my suspicion that the case was one of *constitutional lues*. The patient assured us, however, that it was upwards of *sixteen years* since he had had chancre. On inquiring into the particulars of that seizure, he told us that, being very young, and totally unskilled in such matters, he had applied to no surgeon, but had attempted to remove the sore by washing with cold water, and keeping it clean; and that it continued increasing in size for three or four months, when, finally, it was healed in a few days by the application of a little mercurial ointment. The glands were not affected, he said. This account did not remove my suspicion, but it impressed Mr. B.'s regular medical attendant, who was a man of sense and ability, with the conviction that a secondary syphilitic affection could not exist, considering the distance of time and other points; and accordingly the case was still treated as one of a rheumatic nature.

I ought to mention that my chief reason, in addition to the affection of the shin and the pains, for suspecting the complaint to be syphilitic, was this:—several months previously to the occurrence of the swelling on the shin, I had been bathing on the open sea-shore with Mr. B., and had accidentally noticed a brown scab upon the preputium. I cautioned him about it, and was informed by him that similar scabs came upon the prepuce almost every summer, but that he paid little attention to them, as they gave no annoyance and went away of themselves. In consequence of the bathing on the day alluded to, however, the scab then existing fell off, and an ulcer of considerable size, with hard edges, was the result. This sore spread, and resisted simple dressings. When treated ultimately with strong mercurial ointment, it disappeared. It seemed to have been caused by a number of small pustules running together. These circumstances I recalled to the memory of Mr. B. and his medical attendant, but, as I have said, the case was still held by them to be one of simple rheumatism. The shin was leeched and blistered, and, in consequence, the ridged appearance and the pain on pressure were

removed. The patient was at this time put under a slight course of mercury, also, though this was not kept up long, being intended merely as an alterative, to relieve some symptoms of disordered digestion. Nevertheless, the pains in his groins and thighs continued all winter, and they were occasionally very severe in his ancles. In the following summer they were considerably alleviated, and at this time, as an alterative, he took decoctions of sarsaparilla for a very short time. In the subsequent autumn the pains again returned, and his right shin showed a similar affection to that which had attacked the left. Leeching and blistering again removed it, and the general health was much improved by a short residence, as was thought, at a watering-place, famed for rheumatic cures. During the following winter, Mr. B. was very careful of exposure, and enjoyed considerable exemption from pain; but his limbs, above the ancles, had a sort of hide-bound feeling, very perceptible to him for some time.

Of the two subsequent years little need be said. He used cold spunging and cold bathing freely, and suffered only now and then from pains. At the same time he was unhealthy in aspect, and had frequent attacks of dyspepsia. Upwards of a year back, however, at the beginning of autumn, the pains returned with intense severity to the thighs, and ultimately both shins became so much swollen as to disable him entirely from moving. The pains also shifted down to these regions. It was at this period that I got charge of the case, the former medical attendant having removed to an inconvenient distance. A new and more minute examination convinced me fully of the correctness of my former opinion. The periosteum and soft parts of the right shin were much swollen, and seemed about to suppurate. The other shin was similarly affected, but less severely. Both tibiæ were ridged anteriorly and laterally, for a considerable way above the ancles, and, in short, had a series of small nodes upon them, not to be mistaken. On looking at his throat I found it to be *ulcerated* in one or two points. I also discovered the clavicles to be much enlarged, though the only pains felt in that region were confined to the tops of the shoulders and the sternum. But these pains were slight; and about this same period there also appeared a puffy tumour on one temple, with a few ulcerated specks on the scalp. I immediately applied leeches to both limbs, and afterwards blisters, giving the patient about thirty drops of a solution of muriate of mercury (two and a half grains to the ounce of distilled water) twice a day. The bleeding and blistering relieved the swollen and puffy appearance of the parts and the local pain. But the nodes remained undiminished, and, as soon as the sores consequent on the treatment mentioned were healed, the legs were subjected to daily mercurial fumigations. A pint of decoction of sarsaparilla was also ordered daily, along with the mercurial drops. In the course of the first fortnight, the nodes gradually began to disappear, and the patient experienced little or none of his customary pain. As the mercury began fairly to act on the system, matters still continued improving, though I took care, during the whole later use of the mercury, to keep it below the point of salivation. Mr. B.'s general health rapidly bettered. The healthy complexion, which had been banished from his cheeks for three years, was again restored. To sum up the case briefly, it may be mentioned, that the fumigations and drops, with sarsaparilla, were used for upwards of four months.

Before that period, however, the patient was quite well. All appearance of nodosity was removed, and every feeling of pain dispelled. In all, the quantity of mercury used in fumigation was 26 drachms, of which 40 grains were employed daily.

I have allowed eight months to elapse before calling attention to this case, in order to learn if the benefit was likely to be lasting. I am not now in Mr. B.'s neighbourhood; but I may merely repeat the words which he himself uses in answer to inquiries, "I am strong as a lion." In no point has he the slightest complaint—a novelty to him for many years.

I think, sir, that this case is of some consequence, as throwing light on many of the cases usually called rheumatic, which occur in the instance of persons advanced into complete manhood, and who, from the length of time that has elapsed from the original attack, never once suppose that their affections are of a secondary syphilitic nature. That Mr. B.'s was of this nature can scarcely be doubted, I think, although sixteen years had passed away since he incurred its origin. It is to be observed, however, that the pustules on the prepuce were appearing often in that time, and that he had been affected with pains, though slight, for many years back. The length of time, indeed, between the occurrence of the chancre and the first distinct seizure with pains, was, properly speaking, not more than eight or ten years. His statement, that he had no *new* affection in the interval, may be fully depended upon —such, at least, would be the opinion, I know, of all who are acquainted with his honourable character. Such being the case, I would have all parties who feel pains of a doubtful or dubious kind, after they advance considerably in years, to have their cases narrowly looked into, and not to trust to the lapse of time as securing them from all chance of being syphilitically affected. Let them look to their clavicles, and look to their throat. A venereal ulcer is of so sluggish a kind that it may exist there

unnoticed for years. I am convinced that many men are cut off in mid-life from this cause, who might be saved if their true complaint had been discovered, and actively treated.

One eminent Scotsman, I feel convinced, perished from this cause. The severe and anomalous pains which he is represented as having suffered before his death, were exactly similar to those in Mr. B.'s case, and in others that I have seen. The name of that great man the reader must discover for himself, for I cannot bring my mind to the open mention of it in connection with this subject.

Trusting that this case may be a useful warning, I remain, Sir, your most obedient servant,

JOHN COWAN, Surgeon.
2, Roxburgh-street, Edinburgh,
6th December, 1839.

SIR CHARLES BELL.

To the Editor of THE LANCET.

SIR:—Being a graduate of Edinburgh, and having a diploma from the College of Surgeons there, I take the liberty of addressing you, for the purpose of correcting some statements which appeared in your Number for the 23rd of November, regarding the manner in which the chairs of the Edinburgh University are filled.

No one who is acquainted with the valuable works of a practical nature which have been given to the public by Sir Charles Bell, and with the fact of his having been actively employed as a surgeon to the Middlesex Hospital for a very long period, can reasonably doubt that it was by the reputation gained by those works, and that practice, that the Town Council were guided, and not by his justly great physiological eminence.

As regards your remarks on the falling-off in Sir Charles Bell's class (setting aside, of course, the first year, which, from various causes, always shows the largest numbers), the decrease has not been such as to warrant your using the expression of *fast diminishing;* and the reasons for that decrease, which can be proved to have taken place, are, partly the general decrease of numbers in all the medical classes, and in a far greater degree the regulation passed by the Edinburgh College of Surgeons, by which a course of military surgery, of three lectures weekly, shall be allowed to stand for one of the two prescribed surgical courses. The temptation thus given to students, a great proportion of whom are looking to the army, need not be dwelt upon.

Further, with regard to the students' opinions of merit in a teacher, Edinburgh, I grieve to say, shows too often how they treat those professors whose lectures they do not value; and I can affirm, that during three sessions that I have attended the medical classes of Edinburgh, that of Sir Charles Bell was, with one exception (that of Dr. Christison), the most distinguished by order and attention on the part of the students.

Trusting that the desire to have justice done to Sir Charles Bell will excuse the trouble I have given you, I remain, Sir, your obedient servant,

VERUS.

London, 25th Nov., 1839.

*** We adhere to the opinion that Sir Charles Bell obtained the chair at Edinburgh from the same reputation and kind feeling which procured for him the honour of knighthood—with the additional recommendation that the councillors, in selecting their countryman, elected a titled professor into their University. Nine-tenths of the profession, without derogating from his merits as a surgeon, will coincide in this impression. With regard to the alleged "failure" of his election, as a means of reviving the attractions of the University, our statement is unshaken. The classes have *decreased.* The objectionable system of election shall willingly have the full benefit of our correspondent's reasons for the diminution. If the students of Edinburgh are rude in their demonstrations of dissatisfaction with incompetent teachers, we must regret the circumstance, for politeness is cheap enough; but the same judgment which leads them to pay due respect to the great talents of Sir CHARLES BELL, warns them to be angry at worse appointments.

DISPENSARY FOR THE DISEASES OF THE LUNGS.

MR. GRAY AND DR. SHERRIFFS.

To the Editor of THE LANCET.

SIR:—My wit seems to have cruelly afflicted Dr. Sherriffs with the same disorder, for I am not only witty myself, but the cause of wit in others, which your readers will easily perceive without the aid of spectacles. In the reply of Dr. Sherriffs to my letter on his Lung Repertorium, he has advanced a truth in the preceding portion of his reply, establishing my superlative shrewdness and nice discrimination in detecting hypocritical schemes. He says "Oh, no;" but I say "Oh, yes;" for I do walk abroad with my eyes open, and have much excellent vision to boot, also amongst my other varied endowments. I am a capital hand at somnambulism, so that, asleep or awake, my judg-

ment is never at fault; and such a nose for smell, that I can discover filth from my residence as far off as Charlotte-street, Portland-place. Dr. Sherriffs, in his desire to establish my character on the basis of imperishable renown, has, with a happiness of diction and a fine feeling of charitableness toward me, stated that I am glaringly deficient in that article called truth: this is delightful beyond measure. Now, I shall endeavour to dissect Dr. Sherriffs, with the view, if possible, to discover whether that jewel, truth, really exists in him in such size as entitles him to correct the wanderings of my imagination. First, he denies that he ever put forth a pretension to cure consumption in every case. Now for the proofs of my statement. In a fly-leaf annexed to the Prospectus of his Lung Sputum, I find these words:—"In the press, and shortly will be published, 'Consumption Cured; or, a Letter to the British Public on the Mortality, Causes, Successful Treatment, and Prevention of Consumption. By E. B. Sherriffs, M.D., F.R.C.S.E., Consulting Physician in Diseases of the Lungs, formerly Lecturer on Anatomy and Physiology in the College of Edinburgh." The medical world was not aware that Dr. Monro had resigned the chair of anatomy and physiology in favour of Dr. Sherriffs; however, this is nuts to crack for Christmas. Now, Sir, he here plainly states that he cures and prevents consumption. What does this amount to? He does not say in certain cases; he asks not for selected ones. Is it not a logical syllogism, that if he cures and prevents consumption, of course he does so in all cases? If he fails to cure and prevent consumption, his boasted puff falls to the ground. If Dr. Sherriffs had, in his Letter to the British Public, said that he had discovered a mode of treatment by which, in certain cases of consumption, a cure would be realised, of necessity I never would have dared to attack him; but to cure and prevent is such an Herculean task that it stifles my imagination, and the attempt to swallow such a huge comfit is so appalling to gullets of small size, such as mine is, that, with all imaginable respect to the worthy proposer, I declined the undertaking, and he finds fault with me for resisting his elephantine bolus. So much for my first lie.

Now for the second. "Mr. John Gray," he says, "continues his veritable relation that Dr. Sherriffs states he is the only physician in London who understands and restricts his attention to consumption." Dr. Sherriffs relates another most palpable falsehood. "I never gave such intimation." Well, there is nothing like facing danger in a bold manner; it shows that you have the warranty of rectitude to back you, or, on the contrary, an awful stock of impudence; this last commodity the Doctor possesses to satiety, which I shall show by the following abstract from his Lung Elysium:—"From the circumstance of Dr. Sherriffs being the only physician in London who restricts his whole attention to the study and practice of diseases of the lungs, the directors beg to observe that the patients will have the advantage of a more exact study and a more ready discrimination of their cases, than could be so conveniently afforded by physicians engaged in more general practice."!!! What is this tantamount to?—The only physician—a more exact study—a more ready discrimination, than by other physicians. Is it not equivalent to understanding diseases of the lungs better than any other physician in London? And yet he has the effrontery to deny that he ever gave any such intimation, and to lay to my name the stigma of falsehood. As to his other assertion, that I am utterly unpractised in the conduct of a gentleman—there, indeed, he may be right, for as to the term gentleman, considerable perplexity has enfolded me regarding its proper acceptation. But this I can tell him, since ever I had the honour of being admitted to the fellowship of the medical profession, a conquest which I performed by my own exertions, from poverty having risen to respectability, not by the aid of *ad captandum* puffs and bare-faced assertions of superior skill, but by the steady progress of sound esteem and mutual confidence on the part of those who did me the honour of requesting my aid in their ailments, whether of wind or limb,—I have conducted myself as becomes my calling.

In reading over the prospectus which has been issued by Dr. S., I find these words:—"But it is a melancholy truism, that in London 13,399 lives are annually sacrificed (never more nor less) to this heavy affliction to humanity." Underneath is this note:—"It is fair to observe, that the report of the Registrar-General, a report based on information generally derived from the most ignorant of the population, or from persons wholly unacquainted with the nature of disease, consequently, a report of no value as relating to the causes of death, gives the annual mortality in London, from consumption and decline, at the rate of 7,754 per annum." This is a sample of elegant composition that none but Dr. Sherriffs could have accomplished. Ye medical men of London, here is a dose for you, gratis!—you that give in reports of all the causes of death where you have attended. Dulcet words! Most ignorant—wholly unacquainted with the nature of disease—consequently your statements are of no value! For how can they be when this transcendant genius contrives to multiply 7,754, like Falstaff's men in buckram, into 13,399? Where he got the fractional 5,645, to make up his sum-total of 13,399, I cannot divine; but he is no common man, and that is a truism too. As I proceed, I find that there

is at present, incredible as it may appear, no institution in London for the medical treatment of diseases of the lungs. None, indeed! I recollect one in Union-street, Bishopgate; I happen to know something of that too, for I used to dole out the wash of physic to the poor creatures who came for relief. May God forgive me for being accessory to the fact! He allows of one in Artillery-place, for asthma; I believe that has something to do with the lungs. Besides, does not every hospital and dispensary in London administer advice and medicine to every one who applies, with lungs or no lungs at all? No, no, Doctor; don't attempt to fly before you are fully fledged; you aim too high; besides, who do you think can afford to give you five pounds per annum to become subscriber? It is, by far, too much. What think you of coming down in your price—say five shillings per annum? You will be able out of that to stuff yourself occasionally with a capital haggis or a singed sheep's head. I have no ill-feeling towards you, believe me, for all my banter; but I am determined, whenever an opportunity exists, of attacking false principles; and on the least semblance or bare approach to quackery, in the regular and well educated medical man, I shall, at all hazard, do so. I now conclude, with this short intimation to "Old Subscriber," that I know more of the Lisson-grove Asylum than he seems to know. I happen to have a very curious thing in my possession that did the Asylum a world of good, when in existence. At present I beg to remain your humble servant,

JOHN GRAY.

7, Upper George-street,
Bryanstone-square, Dec. 14th, 1839.

THE LANCET.

London, Saturday, December 21, 1839.

IT is difficult, in the present day, to keep pace with the progress of Medical Reform. Every week we have to announce the formation of an association, the appearance of a letter, the publication of a pamphlet, or the accession of a convert to the cause, all more or less favourable to the establishment of liberal principles upon the ruins of the self-elective corporations. The day of action is come. Medical practitioners have a clear idea of their rights; and they now demand, with an ardour which cannot be suppressed, a controul over their funds, the right of self-government which has been conceded to the municipal corporations, and the guarantee of privileges which have hitherto been but a delusive mockery. To sit tamely down under the TWENTY-ONE self-elective Councillors of Lincoln's-Inn-fields, the Blackfriars Company, or the insolent Fellows of Pall-mall, is felt to be an indelible disgrace. Helotism is no longer held to be honourable. Even top-apothecaries no longer hug their chains. Physicians, surgeons, general practitioners—call them what you will—see that it is just that every well-educated man in the profession should enjoy the titles and honours to which his talents and industry entitle him; that whether living in London or in the Country, the highest offices should be thrown open to every honourable ambition; that nephews and noodles should no longer thrust aside the ablest practitioners in the kingdom; and that instead of nineteen feeble, isolated, conflicting, irresponsible Examining Boards, the profession should be organised in one representative Faculty, which would shelter the profession from oppression, break down monopolies, and, at the same time, protect the public from the impositions of impudence, ignorance, and quackery. Honours and rewards they are convinced should be heaped upon the men who serve the public by the free suffrages of their brethren, and be the marks of talent and virtue, instead of wreathing the brows of imbecility, or glittering upon the breasts of apostacy. In promoting the science of medicine, in securing the diffusion of its advantages among all classes of the community, the poor as well as the rich,—in upholding the rights of its professors, and in doing justice to those who have deserved well, who will not join? In the glorious cause of Medical Reform, who can remain neutral and guiltless? Let that man hold up his recreant hand and secure the derision of mankind.

Mr. RICHARD JONES, of Leamington, has recently penned a small volume of *Observations on Medical Education, with a View to Legislative Interference*,* which he has dedi-

* Murray: 1839.

cated, "by permission," to Sir ROBERT PEEL. With the greatest respect for the motives which inspired Mr. JONES—or at least induced that gentleman—to write upon the subject, we must not consider the acceptance of the Dedication as a proof of Sir ROBERT'S regard for the medical profession, or of his desire to see its institutions placed upon a satisfactory footing, or even of an implied approval of the principles or plan of the speculator. Sir ROBERT PEEL declared, in a communication to the representative of a large body of surgeons, several years ago, that he never would consent to a reform in medical law which should give to the members of the College of Surgeons the elective privileges of an intelligent body of men. But he may *by this time* have changed his mind, for he has since pronounced in the House of Commons the somewhat inconsistent eulogy, "that for the "enlightened views, pure philanthropy, and "liberal feelings, of medical men, generally, "he felt so much respect, that he did not "hesitate to pronounce them a blessing to "their native land, and an honour to hu-"manity." This is the language of panegyric, but we will hope that it was spoken in perfect sincerity, for if Sir ROBERT PEEL really regard the medical profession with friendly feelings, he will overlook neither their interests nor their grievances. When Medical Reform is mooted, and fairly brought before the House of Commons, in the next session of Parliament, and he has applied his mind (which in legislation all must admit is very able) to the subject, he should contribute to the settlement of the medical institutions of the country upon a liberal, solid, permanent basis. This is a matter which concerns the public health, and should not be considered as a party political question. The Honourable Baronet cannot refuse the members of a profession, for which he really feels "so much respect," the right of self-government, or the representative responsible councils which he has conceded not only to English and Scotch, but to Irish boroughs.

Mr. JONES's "plan" is "submitted to the consideration of the medical body" in the following proposition:—

"After the proper course of education has been completed, and attested, by the admission of the candidate as a member of the College of Surgeons, and a licentiate of Apothecaries' Hall—before he shall be permitted to practise, on his own account, he shall be compelled, by law, to pass a probationary period of two years with some practitioner who has been in practice ten years—taking that as the minimum period; and that, at the expiration of this time, a license or qualification to practise shall be granted him from the government, on the testimony of the practitioner with whom he has probationised—which may be further attested by the magistrate of the county."

But if the "admission as a member" of a national medical examining body take place only as a consequence of evidence that the candidate is competent to practice medicine, there can be no need to damn that guarantee by any subsequent "probationary period"—the observance of which would soon become only a pretence and a mockery.

Mr. JONES throws out several hints which might not be altogether useless. But his plan is very imperfect. It does not go to the root of the matter. His views are circumscribed. He has thought but superficially upon the subject himself; and, if we judge from the quotations and the notes, his reading has been neither extensive nor select. Many of his authorities are of the worst description. In advocating reform he cites the organs of anti-reform approvingly. Now if the Devil quote scripture, honest Christians should be wary in citing *his* books.

The "British Medical Association," and the plan of "One Faculty," however, meet with the writer's approval.

"The annual report of another body, called the "British Medical Association," has just been published. The views and objects of the Society, as announced by it, on the subject of medical education, are much in unison with those stated in the preceding pages; and exactly accordant with my own views, is the further proposition of one faculty of medicine—one uniform system of qualification—and the establishment of a general medical senate, for the purpose of protecting the rights and privi-

leges of the medical body, and of assisting the government on all subjects connected with the public health."

"It is scarcely to be expected," he says, "that much addition can be made to the "improvement of either practical or scientific "medicine" at the meetings of the Provincial Association. And the author entertains sanguine hopes of the success of Medical Reform in parliament.

"In concluding these observations, it is encouraging to remark that the time is evidently fast approaching when the whole subject of medical education will be brought under parliamentary notice. Mr. Warburton and others, in the House of Commons, have already thrown out intimations of some intended movements on the subject; and the unusual circumstance of the editor of a medical periodical* being an active member of parliament, favours the probability that inquiry will not long be delayed. * * * It is, also, another remarkable circumstance, that medical men are now, for the first time, so generally coming forward as candidates for the office of coroner. Let it be added, that the periodicals teem with observations, forcibly expressed and urgently repeated, on the necessity of placing on proper grounds, under legislative sanction, the whole system not only of medical education, but also of medical practice. All these circumstances, indicating the improving spirit of the time, and emanating from it, are, in the highest degree, auspicious to the hopes of every well-wisher to the health and the happiness of mankind."

DIFFERENT FORMS OF THE BLOOD PARTICLES IN THE MAMMALIA.

M. Mandl, by his late discovery of the elliptical corpuscles in the blood of the dromedary, has demonstrated that the circular form is not universal in the blood particles of mammals; and Mr. Gulliver, in a paper read at the Royal Medico-Chirurgical Society, an abstract of which was published in a late number of The Lancet, has shown that similar elliptical corpuscles belong to the blood of the different species of *auchenia*, and has given a minute account of the dimensions of these oval disks.

Mr. Gulliver informs us that, in pursuing his inquiries, he has discovered a form of corpuscles in the blood of mammals, which is not only new to that class, but which does not appear to have been described as existing in any animal whatever. These corpuscles are of a lunate or crescentic form, sometimes lanceolate, and are altogether so singular and distinct in their figure, that there is little doubt but that they must be connected with some peculiarity which has yet to be discovered, in the organization of the animal. Mr. Gulliver found them particularly in the blood of the *cervus mexicanus*, and the *cervus porcinus*. He informs us that he will give a particular description of these peculiar corpuscles, as soon as he can get the necessary illustrative figures completed.

* Mr. Wakley.

SUSCEPTIBILITY TO MERCURY.

To the Editor of The Lancet.

Sir:—I have taken the liberty of forwarding, for insertion in your valuable Journal, the following case, which is interesting, as exemplifying a peculiar susceptibility to the action of mercury:—

X. Y., aged about 25 years, applied to me for relief for a severe tooth-ache. Upon examining his mouth, I found a small excavation in the crown of the second bicuspis of the upper jaw; the toothbone was firm and healthy, the enamel only slightly injured, and the surrounding gum and mucous membrane unaffected. Under these circumstances, I advised the stopping of the cavity, with a view to saving the tooth, an operation which I have performed with success in numerous instances. The preparation which I used upon this occasion, for that purpose, is an amalgam of silver with mercury; the quantity introduced into the hollow of the tooth was six grains, of which one-fourth was metallic mercury.

In twelve hours the amalgam had become firm, and the nerve, protected from the influence of the external air, had ceased to be painful.

In twelve days from this time I was sent for to this man, and found him labouring under severe ptyalism; I removed the stopping from the tooth, and, upon inquiry, no other cause could be ascertained for this attack than the small proportion of mercury

thus brought into contact with the salivary fluid. His constitution was good; he was a married man; had never suffered from disease of any kind, and had occasionally taken a blue pill, as an aperient, without suffering any inconvenience.

Under these circumstances, and from careful inquiry with regard to his preceding habits, aided by the opinion of the physician attending the patient, I have been obliged to come to the conclusion that this must be a case of peculiar idiosyncracy. I am aware that the smallest proportions of any preparation of mercury will in some individuals produce the characteristic effects of that medicine, even when simply brought in contact with the external surface of the skin; but I have never before met with one so strikingly marked as the preceding. I trust that this case will be thought interesting to your readers, some of whom may have met with others equally remarkable. I need not say that such instances are important to the practitioner, for by occurring to a young man, they might be calculated to severely injure his reputation and prospects of success in practice. I have the honour to be your obedient servant,

GEO. WILSON.

100, Great Portland-street,
Dec. 10, 1839.

MEDICAL SOCIETY OF LONDON.

Monday, Nov. 2, 1839.

Mr. HEADLAND, President.

THE ACTION OF ARSENIC AS AN INTERNAL AND EXTERNAL REMEDY—IS IT AN ACCUMULATIVE MEDICINE?

A MEMBER inquired the experience of other members with reference to the internal use of the white oxyde of arsenic in uterine diseases, and also of the liquor arsenicalis, as an external application. He was induced to ask this question from having known cases in which severe gastritis was induced by the practice alluded to. In one of these cases only four or five doses of oxyde of arsenic, consisting of a twentieth part of a grain, were administered, and in two other cases the liquor arsenicalis was applied as an external application, in one instance to the eruption of lepra, and in another to some warts.

Dr. BENNETT had seen many cases in which arsenic had been used externally by Dupuytren, who was in the habit of applying a dossil of lint, which had been dipped into liquor asenicalis, to ill-conditioned ulcers, for the purpose of producing a slough, and engendering a more healthy action in the sore. Symptoms of gastric irritation were always carefully watched for. He (Dr. Bennett) had not seen any ill consequence result from the practice. He had no doubt that the white oxide of arsenic would, even in minute doses, act very injuriously on the mucous membrane of the stomach. Dr. Christison had asserted that a single grain of it might produce fatal effects.

Mr. DENDY considered that the practice of using arsenical preparations for cutaneous eruptions was highly reprehensible. Other remedies, of a less dangerous character, could be used for the same purposes, and would produce the same stimulating effects, as the nitrate of silver, &c. It was well known that the application of medicines to an abraded surface of the body, was one of the most speedy and effectual modes of influencing the system. He thought, with reference to the white oxyde of arsenic in substance, that in its internal use, its effects were much less controullable than those of the liquor arsenicalis, and it was therefore not so advisable a remedy as the latter. He did not believe that the liquor arsenicalis was an accumulative medicine, but considered, when commenced in small doses, it might be carried on safely until some disturbance of the digestive organs was manifested.

Mr. KINGDON was always cautious in using medicines like arsenic, and he seldom ventured beyond doses of two or there minims of the liquor arsenicalis. With reference to the local application of the liquor arsenicalis, he believed that it was not simply a stimulant, but had, in fact, a specific action, which could not be produced by any other medicine. Mr. Kingdon then related the case of a woman with a schirrus tumour of the breast, very nearly on the point of ulceration, who applied to him when he was surgeon to the City Dispensary. He applied to the tumour a plaister of common cerate, containing as much of the liquor arsenicalis as could be rubbed into it, which, after being continued a length of time, succeeded in separating the diseased growth completely, and the woman recovered. The process, however, was so painful and tedious, that he should never be induced to adopt it again.

Dr. BENNETT has seen Dupuytren apply arsenic as an external application in many cases of cancer, after the commencement of ulceration. The preparation he used consisted of one grain of white arsenic, mixed with 199 grains of calomel, and of this about ten or twelve grains were sprinkled upon the ulcerated surface, which was covered carefully by a simple piece of plaister. The plaister was not removed until the discharge had become so copious as to render it necessary to cleanse the sore. The application was then repeated. He, Dr. Bennett, recollected no instances in which any bad constitutional effects resulted from the

application, which, however, sometimes disagreed with the local affection, and required to be discontinued. The object of the remedy was not only to produce a slough, but to induce a specific action on the vessels of the part to stimulate them to more healthy action.

Mr. DENDY referred to an interesting case, published by him some years since in THE LANCET, in which a small tumour "shelved out" from the assiduous application of the tincture of iodine.

Mr. ROBARTS had used the liquor arsenicalis to a considerable extent in a case of *noli me tangere*. He employed the remedy for nearly eight months, with short intermissions, on some occasions the dose being carried to the extent of eighteen minims three times a-day. The patient was cured. He had found the medicine not to be accumulative; but its injurious effects upon the stomach were gradual, and he always left off the medicine when the symptoms indicated the necessity for such a step.

Dr. CHOWNE believed that arsenic was not accumulative. Persons had attempted to poison by degrees, by administering this medicine in minute doses, and thus gradually undermining the health. The victim has been thus affected to a certain extent, but not destroyed. He had seen cases in which larger doses than those used by Mr. Roberts had been taken. It was judicious to commence with small doses. He should never apply a larger dose externally than he should be induced to administer by the stomach.

Monday, Dec. 9, 1839.

Mr. PILCHER, President.

IS IODIDE OF POTASSIUM, OR THE PREPARATIONS OF MERCURY, OF THE MOST ADVANTAGE IN SYPHILIS?

THE discussion this evening turned on the relative advantages of mercurials and iodide of potassium, in the treatment of primary and secondary syphilis.

Mr. HEADLAND considered the question to be one of importance; the introduction of the iodide of potassium into practice had made many practitioners doubt whether that medicine or mercury would be most advantageous in certain cases of secondary syphilis. Time was lost in the determination of this point. He had seen several cases of syphilitic disease occurring in medical men, who were particularly undecided as to what course they should adopt. He thought the Society should point out the particular cases in which the iodide of potassium would have the advantage of mercury. In some hospitals the mercurial plan of treating syphilis was carried on to as great an extent as it had ever been, while in other institutions, of a similar kind, even primary syphilis was treated as though it were merely a local disease.

Mr. PROCTOR thought that the particular cases of syphilis, or pseudo-syphilis, in which the hydriodate of potash or mercury would be most useful, could not be pointed out; sometimes one remedy was the better, sometimes the other, while in some instances a combination of the two was the most effectual.

Dr. T. THOMSON believed that where iodide of potassium was employed as a remedy in the primary symptoms of syphilis, its effects were not so marked and permanent as they were when it was used in the secondary form of the disease.

Dr. CHOWNE was decidedly in favour of mercurials in the treatment of the primary symptoms of syphilis. He thought that the reason of our meeting with the secondary symptoms of this affection more frequently of late years than formerly, was in consequence of mercury having been less employed than it used to be. With regard to the secondary forms of syphilis, however, it was first of all an important question to decide which cases were those of secondary syphilis. Medicines, he believed, often got an undue celebrity in the treatment of diseases, the diagnosis of which had not been accurately made out. In the treatment of syphilis, a mild course of mercurial medicine he thought most advisable. In cases, however, when, in consequence of the treatment, the constitution had become affected, then iodide of potassium might be employed with advantage.

Mr. DENDY considered that, with judicious management, the best medicine we possessed for syphilis was mercury. A variety of remedies had been attempted to be substituted for this, in the treatment of the disease in question. Nitric acid had been some years ago in much repute, and now the iodide of potassium was considered by some as a specific.

Dr. HUGHES said, that at Guy's Hospital mercury was still exhibited as a remedy for the primary symptoms of syphilis; the preparations of iodine being confined to cases of the secondary forms of the disease, and to those instances of the primary affection which occurred in constitutions which would not bear the exhibition of mercury. If secondary symptoms occurred less frequently after the employment of mercury than when other remedies were used, these symptoms were, nevertheless, of a more severe character. An old physician informed him, that he had never seen a case of the secondary form of the disease, affecting either the palate or the bones, unless mercury had been administered. Cases of primary syphilis seldom occurred under his (Dr. Hughes) care; cases of secondary symptoms, however, constantly came under his notice. When, in these, there was ulceration of the throat, or a scaly eruption of the skin, the iodide of potassium with iodine

was a valuable remedy. It was, however, sometimes exceedingly capricious in its effects. He had attended a strong, healthy young man, with several large ulcerations of the scalp, and blotches of scaly eruption on the back and shoulders. The nitric acid lotion was applied to the former, while iodide of potassium was administered for the cure of the latter. No benefit, except a rather more healthy appearance of the ulcers, resulted. He discontinued the remedies; but, a short time after, the very same preparations, and in the same doses, were ordered by a surgeon, and, in the course of ten days, both forms of the disease were removed.

Dr. Bennett had found the iodide of potassium of great use in cases of nervous headache, in which there were no symptoms of disturbance of the circulation. One of his colleagues employed it in neuralgic pains, particularly those occurring in hysterical girls, with so much success, that he was fearful of publishing the cases, lest they might not be believed. He (Dr. Bennett) considered that the good results of the medicine were to be attributed to its tonic effects upon the capillary system.

Dr. Thomson had employed the iodide of potassium with benefit in cases of neuralgia which were of a rheumatic character, but not, strictly speaking, rheumatism.

Mr. Procter believed that the preparation under discussion was not a tonic, but interfered seriously, in some cases, with the functions of the digestive organs.

Mr. Headland should say, as a general rule, that, unless this medicine were given for its *specific* effects, it should not be employed in cases where atrophy existed; but its use would be of much advantage in all cases of hypertrophy, such as periostitis, &c.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, Dec. 10, 1839.

Sir B. C. Brodie, President.

Remarks on Emphysema of the Lungs. By George Budd, M.D., *Physician to the Seamen's Hospital Dreadnought.*

Dr. Budd commences his paper by detailing the principal anatomical characters of emphysema of the lungs, which were first distinctly pointed out by Dr. Baillie, and which have since been more fully described by Laennec. It is one of the objects of this paper to show that want of elasticity in the lung, in other words, absence of its natural tendency to collapse, is the cause of many of the other anatomical characters of emphysema, and of most of the symptoms by which this affection is characterised.

The symptoms and anatomical characters which Dr. Budd ascribes to this cause are, the cylindrical form of the chest, and elevation of the shoulder-blades and collar-bones; the abdominal character of the breathing; the immobility of the parietes of the chest during the acts of respiration and cough; the peculiar character of the cough, which is short and interrupted; the dryness and diminished vascularity of the capillary system of the pulmonary artery, in portions of lung affected with emphysema: imperfect arterialization of the blood, and consequently diminution of animal heat; obstruction to the circulation through the lung, causing dilitation of the right cavities of the heart, and a tendency to general œdema.

Dilatation of the air cells was considered by Laennec as the chief character of emphysema of the lungs, and he supposed that this dilatation was caused by some obstruction in the air passages, which prevented the free escape of air from the lungs. The explanation given by Laennac was generally received, until doubts of its correctness were excited by the discovery made by Dr. Jackson, that emphysema is very frequently an hereditary disease. These doubts have been confirmed by the accurate researches of M. Louis, who found that emphysema often developes itself without the occurrence of pulmonary catarrh. Dr. Budd considers dilatation of the air cells, like dilatation of the chest, a necessary consequence of want of elasticity in the lung, which he regards as the fundamental and primary character of emphysema.

Dr. Budd gives an account of the state of the lungs in twenty horses of various ages. He concludes, from these dissections, that emphysema of the lungs is very common in horses, and that in them it follows, in all particulars, the same order as in man. He infers from this, that the disease has the same cause in both; and that, in horses as in man, it is very frequently hereditary. The paper concludes with some general remarks on asthma.

Dr. B. speaks throughout of "vesicular emphysema;" what has been called "interlobular emphysema," that is, extravasation into the cellular tissue of the lung, rarely exists to such a degree as to give rise to any symptoms, or to merit being considered as a disease. It is the result of an accident, namely, rupture of the air cell, most commonly caused by a deep and rapid inspiration, and which is, generally, a very trifling injury; the density of the cellular tissue of the lung preventing the extravasation taking place to any great extent.

Dr. Johnson inquired how the author reconciled his opinion, that the bronchial tubes, although elastic, were not contractable, with the occurrence of a fit of asthma, its relief after a few hours, and the occurrence of the fit again with equal violence. Why, he would inquire, was the elasticity

in these cases lost, and acquired again successively in a few hours? Dilatation of the air cells did not always occur as a sequence of asthma, but might be found in cases in which that disease had never existed. He recollected, some years ago, examining a gentleman, who had an habitual dyspnœa, a kind of perpetual asthma, for several years. A very extraordinary state of the lungs was found to exist, the air cells being dilated in various parts, from the size of a pea to that of a small pear. Mr. Thomas was present at that examination, and remarked that the lungs of the great Dr. S. Johnson, which he had inspected after death, were diseased in a similar manner. It was well known that Dr. Johnson was not at all asthmatic, not having had any regular attacks of that disease, although he might be said to be somewhat "broken winded," his breath failing him when he climbed the mountainous parts of the Highlands of Scotland.

Sir B. C. Brodie said, that it was frequently a difficult thing to distinguish the cause from the effect of a disease. Was it not possible that dilatation of the air cells might be an effect rather than a cause of asthma? He recollected that, some years ago, a boy was in St. George's Hospital for hydrophobia, of which he died, having laboured under violent spasms of the glottis. After death the air cells were found dilated in various parts of the lungs, in the shape of small bladders. These were not supposed to have existed before the occurrence of the hydrophobia, and were thought to have been the result of the difficult respiration consequent upon the spasm of the glottis. He knew also the case of a gentleman who had been subject to attacks of spasmodic asthma occurring at intervals, varying from one to six weeks, the seizure apparently depending upon some of the ingesta disagreeing with the stomach. The attacks had now left him for several years. If the disease in this case had depended upon dilatation of the air cells, these must have gradually assumed their natural size. Some years since, he had made a great number of observations and experiments on the respiration of animals. On dividing the eight pair of nerves in an animal, he had been struck with the great resemblance of the symptoms produced to those present in persons affected with spasmodic asthma. The animals appeared to breathe simply by a great voluntary effort, and could not go to sleep, as was the case in some persons affected with spasmodic asthma, when they made great efforts to breathe. He was led to explain this difficulty of breathing in the animals, to the sympathy between the lungs and brain having been divided. The respiration would have necessarily ceased, unless it had been kept up by an intense act of volition, which brought the muscles into action. The author of the paper had alluded to another point, and this referred to the contractile property of the lungs as an agent in expelling air from the air cells. He had no doubt that this contractile power assisted the muscles of respiration to some extent. A French physiologist had remarked that in wounds of the chest the lungs protruded. He, Sir Benjamin, had found this to be the case in animals. During inspiration, the protruded lung returned into the cavity of the chest, but again protruded during a deep expiration, showing that the chest had a contractile power independent of the muscles.

Dr. Budd believed that, in the case of hydrophobia, related by the President, the bladders of air found in the lungs after death, were, in fact, collections of air, which had been extravasated into the cellular tissue. Such extravasations were altogether different from that dilatation of the air cells which constituted emphysema, and was not exceedingly uncommon in cases of croup and hooping-cough. The second case was, no doubt, one of spasmodic asthma. He did not deny the existence of such cases; but he contended that the spasm did not exist in the bronchial tubes, but in the internal muscles of respiration. There was certainly some appearance of muscular fibres in the trachea when viewed superficially, but when seen through the microscope, this would be found not to be the case. He made many careful examinations and experiments, with the view of determining this question. An ox was killed with the blow of a pole-axe; the trachea was immediately removed, and the anterior rings divided. No contraction succeeded to the application of the galvanic fluid.

Mr. Drew made some observations, but in so low a tone that we could not hear them.

Mr. Macilwain believed that the idea that asthma was dependent upon dilatation of the air cells was erroneous. He considered, on the contrary (as we understood), that the disease in question was the result of extravasation of air into the cellular tissue, by which the capacity of the air cells became diminished.

Dr. John Wilson briefly related two cases in which patients perished from emphysema of the lungs, both cases after death exhibiting precisely the same morbid appearances. In one case the sound on percussion was preternaturally clear, but not so in the other; he could not account for this difference.

BRITISH MEDICAL ASSOCIATION.

December 17, 1839.

The minutes of the last meeting having been read and confirmed, the following

gentlemen were admitted members of the Society:

— Dyer, Esq., 8, Minerva-terrace, Barnsbury-park, Islington.

F. Odling, Esq., Oxford-street.

A letter from the North of England Medical Association was read. We shall publish this letter next week, and also one from the Reform Association of Dundee.

A letter was also read from Dr. Maunsell, of Dublin, on Medical Reform, which, together with that from the North of England Medical Association, was referred to the Reform Committee.

The petition of the Association to the legislature, on the subject of Medical Reform, was then read as amended, when it was resolved unanimously, that a deputation be appointed to request an interview with the Marquis of Normanby, her Majesty's Secretary of State for the Home Department, on the subject of Medical Reform.

The deputation having been named, the meeting was adjourned until Tuesday, the 21st of January, 1840.

ST. THOMAS'S HOSPITAL.

NUMEROUS CAVITIES IN THE LUNGS WITHOUT PECTORILOQUY.

JOHN EDWARDS, aged 36, a brewer, in the employ of Messrs. Barclay and Perkins, was admitted on the 22nd of October, under the care of Dr. Robert Williams.

He states, that for the last twelve years he has been in the habit of drinking from six to ten pints of porter daily, but enjoyed good health until four months ago, when, after having taken cold, he felt chilly and shivering; had pains in the abdomen and chest, with cough and dyspnœa. He applied at this hospital, and was treated as an out-patient by Dr. Lister, on the antiphlogistic plan, for some time, but not getting better, he was admitted into the house.

On admission, the most prominent symptoms were dyspnœa, considerable anasarca of the lower extremities, difficulty of breathing, severe pain in the lower part of the left side of the chest, on taking a deep inspiration, harrasing cough, with copious expectoration of greenish frothy mucus. Pulse 100, soft and compressible; bowels opened three or four times daily; poor appetite, but great thirst; free secretion of urine; cough very troublesome at night, greatly disturbing his sleep, and about two or three in the morning copious perspirations come on, which, he says, run over his face, neck, and upper part of chest, as cold as ice; face very pale, its temperature, with that of the whole surface, being reduced.

Physical signs. Voice more resonant than usual between the scapulæ, but as this was not circumscribed, it was ascribed to his emaciation; no marked phenomena on percussion; respiration in upper parts of both lungs gave the idea of the bronchial respiration, conveyed to unusual situations by solidified lung; but there was no dulness, on percussion, to strengthen this supposition. In the lower lobes of both lungs there was vesicular murmer; the sound of inspiration shortened; of expiration, much prolonged. *Apply a blister to the side. Calomel and opium*, of each one grain, thrice a day.

We made no note of the precise length of time he continued this medicine. It afforded much relief. The anasarca went off, probably, from his keeping in bed, and the cough and pain were much relieved by the blister. He got up, and was about the ward for two or three days, apparently a good deal better, when he fancied he got fresh cold.

Nov. 4th. Very pale; respiration hurried; pulse 120; distressing cough, and very large expectoration of frothy muco-puriform matter. The most expressive term for the sounds heard, on applying the stethescope over the anterior superior regions of the chest, is that of a very loud *squashing*, but no pectoriloquy could be distinguished by Dr. Williams, or any of the gentlemen present. The posterior regions were not examined, on account of the exhausted state of the man.

Solution of acetate of ammonia, two ℥.
Comp. spirit of sulphuric æther, one ℥.
Tinct. of hyosiamus, half ℥.
Camphor mixture, one ℥.
Thrice a day.

A grain of opium, night and morning.

He went on, gradually getting worse. On the 13th he was cupped to eight ounces, and a mustard cataplasm applied over the chest. On the 14th two grains of calomel were ordered every night, and red wine with sago; but he continued to get worse, the dyspnœa increasing, and he died on the 17th.

Throughout the case the diffused *squashing* was the most remarkable—very different from the circumscribed sound of gargouillement, but no pectoriloquy could be discovered, even just after free expectoration.

A post-mortem examination was made on the 19th; head not examined. Tissue of upper and middle lobes of right lung, and upper lobe of left, perfectly solid. Not very clear whether this solidification arose from tubercular disease or the effects of pneumonia. These lobes contained numerous cavities, irregular in size and shape, varying from the size of a hazel to that of a walnut. Two of the largest, situated in the posterior part of one lobe, filled with pus of healthy character. The walls of most of these cavities were formed by a sort of cyst, resembling partially coagulated albumen; uniform in consistence and thickness; of a greyish colour, apparently quite unorganised; friable, easily broken down by the finger. The lower lobes of both lungs crepitant, but so much gorged with bloody

serum, probably from commencing decomposition, that no satisfactory observation can be made as to their state. Liver and other abdominal viscera healthy, with the exception of enlargement of some mesenteric glands.

ACUTE CASE OF LEAD COLIC.—NEW MEANS OF DIAGNOSIS.

Jeremiah Bryan, admitted Nov. 5, under the care of Dr. Burton. We shall, probably, notice this case shortly, in illustration of some remarks on hæmatemesis, and, therefore, now merely state that Dr. Burton, anticipating a recurrence of the hæmorrage, ordered the acetate of lead with advantage; but the effects were also such as to afford an opportunity of observing the symptoms of lead colic, as they appeared from the first.

15. The bowels have not been opened since the 11th; the costiveness succeeded by pain, more severe at intervals, with occasional vomiting; after which, it was relieved for some time. It was most severe at the pit of the stomach; like a cord binding the part, and preventing him from breathing freely, extending to the back and abdomen; occasional knotting of the abdominal muscles; disturbed sleep; urine scanty and turbid, voided with some difficulty, but without pain; no pains in the limbs.

20. Much the same. Compound ipecac. powder ten grains, every six hours. The house mixture, of sulphate of magnesia, with infusion of senna, night and morning.

21. Freer from pain; bowels opened three times; vomiting ceased; slept well; skin moist.

Dr. Burton, in making some clinical remarks on this case, stated, that as soon as his patient was under the influence of lead, a deep blue or leaden colour of the gums was observable just where the gum joins the neck of the teeth. He had had thirty cases in the house during the last six years, and it had been observed in all. It was a characteristic symptom of the influence of lead on the system, and likely to prove very useful in the diagnosis of anomalous cases. He believed, also, it was valuable in regulating the administration of the medicine, and by attending to it, the acetate of lead might be given, in such cases as it appeared necessary, without fear of consequences. The principal means of distinguishing the colic of lead from simple colic, was the history of the case; simple colic from enteritis, and other phlegmasiæ, was the state of the pulse, generally about 70, and the absence of febrile action.

The following are the heads of a case, in which this appearance of the gums led to an accurate diagnosis and successful treatment, though the man said he had never exposed himself in any way to the action of lead:—

Cook, a cordwainer, from Guildford, complained, on admission, of a constant pain in the abdomen and loins, which began nine weeks before, accompanied by nausea, disturbed appetite, and occasional vomiting. There was no sign of febrile action; skin cool; pulse 62. He had been laid up seventeen times during the last two years, with a similar affection. Said he never painted or used lead in any form in his business; nor were there lead works in his neighbourhood. The peculiar blue state of the gums was, however, well marked; and Dr. Burton therefore considered that the poison must have entered the system in some unobserved and unsuspected manner, probably by the water he drank.

He accordingly ordered calomel, four grains; opium, one grain at bed-time, to be followed by a dose of house-medicine in the morning. This not operating, he ordered ten grains of Dover's powder every six hours, to be followed by another dose of calomel and house-medicine, and a mustard poultice to be applied to the abdomen. The bowels were opened after this, the evacuations presenting the appearance so peculiarly characteristic of lead colic, being voided at first like sheep's dung; they afterwards became fluid, and light coloured. The linimentum sinapis, applied to the spine, was found a very useful auxiliary in this case. The man has continued to improve.

ST. GEORGE'S HOSPITAL.

OSTEO-SARCOMA OF THE LOWER JAW.

Ann Brookes, aged 31, was admitted into Wright's Ward, under the care of Mr. Walker. On examining her the day after her admission, there was found to be a large tumour of the lower jaw, occupying the situation of the alveolar processes, and extending from the symphysis outwards to the angle of the jaw, having a number of decayed stumps of teeth imbedded on its upper surface. She complained of no pain in the tumour, and her general health was good.

She states that the tumour first appeared about 16 years ago, and increased very slowly and steadily until the period of her last confinement, about 18 months since, since which time the growth of the tumour has been more rapid. Previous to its appearance, she suffered very severely from neuralgic pain in the face, for the relief of which she had several teeth extracted, which gave her no relief. Her entire jaws are filled with teeth in every stage of decay, which have ceased to give her as much pain as before, since the increase in the size of the tumour. She was ordered to have the bowels well opened, and to take some fever

medicine during the first fortnight of her being an in-patient, and a week after that the operation for removing the tumour was thus performed by Mr. Walker:—

An incision was first made from near the angle of the jaw to about an inch beyond the symphysis. Another incision was then made in the median line, commencing just below the lip, and terminating at a right angle below with the former incision. The integuments and muscles were then dissected off the bone, the outer table of which was sawn through, and the tumour removed by the application of a strong pair of cutting forceps. Some diseased teeth, in the immediate neighbourhood, were removed, and some sequestra from the exposed surface of bone chipped off. On attentively examining the mouth, some portion of the disease was discovered far back beyond the reach of the knife or the saw. To this the vitriol cautery was applied freely. Very little blood was lost during the operation; the divided edges of the wound were brought together by the interrupted suture, and the patient removed to bed. Hæmorrhage from some small divided vessels came on a few hours after the operation; the wound was opened, and these were secured. In the evening, a fresh attack of hæmorrhage supervened, which was checked by tying the few remaining open vessels. Some further oozing of blood occurred during the night, but not sufficient to warrant any active measures being taken to check it. Wine and water were frequently administered to her during the night, and a draught of anodyne æther was administered at bedtime.

Three days after the operation, a great portion of the central incision was found to be united by the first intention. As she progressed in her convalescence, the union of the external surface of the wound increased, while the inner margin, and the general mucous membrane of the mouth, remained in a sore state.

A fortnight after the operation the inner and outer edges of the wound had healed and all the ligatures had come away. She was put on a tonic plan of treatment, consisting of quinine, wine, and the most generous diet of the hospital. In the course of a week (being one month from the period of the operation) she left the house with the general outline of the features quite perfect, and no mark of the cicatrix to be seen.

UNIVERSITY COLLEGE HOSPITAL

EXTENSIVE INJURIES TO THE CHEST AND ABDOMEN.

J. C., aged 33, was admitted on the 28th of October, under the care of Mr. Cooper.

He is a stout muscular man, of regular habits. As he was crossing the New Road, a light cart, coming with great speed in the opposite direction, knocked him down, and one of the wheels passed obliquely over his left side. On admission, at 11 A. M. into the hospital, it was discovered that the sixth, seventh, eighth, ninth, and tenth ribs, of the left side, were fractured. No marks of external injury were visible; he breathed with much difficulty and pain; he was pallid; the surface was cold; pulse weak; no expectoration of blood. A bandage having been applied round the chest, the patient was put to bed, and well covered with warmed blankets.

Half-past 12. Not so well; pulse scarcely to be detected; lips livid; surface remarkably cold. Mr. Cooper was sent for. Hot bottles were applied to his feet, and hot brandy and water administered to him, together with ammonia, and other stimulants.

He got gradually worse, and died at half-past one.

After-death Appearances.

The body was examined seventy hours after death.

Head.—Skull and brain uninjured.

Chest.—No external sign of injury.

The left pleura contained some bloody fluid; the membrane was not torn, but blood was extravasated under it. The ribs, mentioned above, were found broken at their greatest convexity; there was extravasated blood in the cellular tissue, external to them; lungs and heart natural.

Abdomen.—Externally no injury.

The peritoneum contained several ounces of bloody fluid, which was diffused throughout the cavity. The left hypochondriac region contained about a quart of blood, partly coagulated. The spleen was ruptured in various directions, and blood formed in its substance, and surrounding it. The left kidney was broken in half, and bathed in blood. The pancreas was literally smashed, and imbedded in semi-coagulated blood. The mesentary contained a quantity of blood.

Liver.—The under surface of the right portion was torn a little, and blood was extravasated about it. The liver was diseased nearly through its entire extent, being hardened, uneven, and tuberculated externally; it was darker in colour than natural, and contained patches of yellow matter; other organs healthy.

STRANGULATED FEMORAL HERNIA. OPERATION.

M. Q., a widow woman, 87 years of age, was admitted on Tuesday, the 22nd of October, under the care of Mr. Cooper.

Has been an inmate of St. Pancras Workhouse during the last three years. Sixteen years since, as she was cleaning a door-plate, she over-reached herself, and felt something suddenly give way in her right groin, accompanied with great pain in the part; on examination, she found a small swelling

there. A surgeon reduced the tumour with some difficulty. She has worn a truss constantly since, but the hernia has frequently come down, its reduction being effected sometimes with difficulty, and sometimes with facility. She has been habitually costive, and yesterday afternoon, while at stool, the hernia protruded, and she was unable to reduce it; she was placed in bed, and during the night, hiccup and vomiting came on. In the morning, the taxis was employed, but without effect, and she was admitted into the hospital at three P. M., when the following symptoms were present:—

A painful sense of tightness at the inferior part of the chest; occasional vomiting of a fluid like coffee grounds; slight hiccup. The abdomen was somewhat tender; the tumour of the size of a pullet's egg, of a round form, was also tender on being pressed. The fundus of the tumour was situated over Poupart's ligament, which was to be distinctly traced above the neck of the swelling; the femoral artery was felt pulsating on the other side. The taxis and warm bath were used, but without success. Countenance natural; pulse hard and irregular; no evacuation from the bowels. Mr. Cooper saw the patient at 7 P.M., and at 8 proceeded to the

Operation—which was performed in the usual manner. The hernia was found to consist of a small intestine, about three inches in extent. Part of this was of a darker colour than the rest, being of a claret hue; but the peritoneal coat of the bowel still maintained its proper glistening hue; the stricture was divided, and the parts placed in apposition.

At ten o'clock P. M., it was necessary to introduce the catheter, to draw off about a pint of urine which had collected in the bladder, and which she could not evacuate.

An enema, composed of water and castor oil, was administered, but it came away soon after, without any fæces; a purgative mixture was more successful, and she had three motions during the night.

The after-history of the case offers little worthy of recording. A small slough formed round the wound, but separated, and the wound healed well by granulation, and on the 19th of November, she had so far recovered, as to be able to walk about.

The case is remarkable only for the great age of the patient, and her recovery by operation, after the strangulation had existed for 31 hours.

CHELTENHAM GENERAL HOSPITAL

Medical Reports, with Cases and Clinical Observations, by JAMES M'CABE, M.D., *Physician to the Hospital.*

PARALYSIS, COMBINED WITH RHEUMATISM.

MARY WALTON, of Charlton Kings, about 43 years of age, was admitted on the 15th of July, 1839. Symptoms on admission:—Inability to move from the recumbent posture, owing to contraction of the flexor tendons of the lower extremities, which obliged her to lie constantly on her back, with her knees raised towards the abdomen. There was a deep and sloughing ulcer on the hip-joint of one side; and, in addition to these complaints, she passed her urine and fœces involuntary. Her illness had been gradually coming on, she said, during six months; but from the perfect rigidity of the tendinous and muscular contractions, it would appear to have been of much longer standing. The following medicines were directed:—

Infusion of roses, ℥ vii;
Sulph. of quinine, gr. xii;
Dilute Sulphuric acid, drops xii;
Sedative solution of opium, drops xx;
Comp. tinct. of bark, ℥ i. Two large spoonsful every fourth hour.
Veratria, 10 grains;
Spermaceti ointment, ℥ j. Rub the ointment over the loins night and morning.

Charcoal poultices were directed to be applied to the ulcer, camphorated oil to be well rubbed into the joints and contracted tendons, and extension of the contracted flexors to be frequently attempted. A nourishing diet, with porter, was given, and great attention to cleanliness enjoined.

These remedies were continued with very little variation, except that an ammoniacal liniment was substituted, after some time, for the ointment of veratria, and the ulcer was dressed with simple cerate, and occasionally touched with the nitrate of silver. Under this treatment, her general health so far improved as to admit of the healing of the ulcer; but as no decided improvement took place in the paralysis of the bladder and rectum, or in the rheumatic contractions of the joints, she was discharged from the hospital on the 3d of October, 1839.

From the history of this case, it would appear that exposure to wet and cold was the original cause of the rheumatic affection, and that the paralysis which succeeded was a consequence of the generally disordered state of the nervous and muscular systems. The permanent contraction of the flexor muscles was, no doubt, occasioned originally by the loss of the antagonist power in the extensors, in the same way as the paralysis of the bladder and rectum followed the diminution and cessation of the contractile power of the sphincters. The case appeared to be a favourable one for trying the effects of the Bath waters, and she was accordingly recommended to the Bath Hospital.

HÆMATEMESIS.

The case of hæmatemesis, given in THE LANCET, still continues in hospital. The

patient has had no return of the hæmorrhage. Having complained much of nausea and uncomfortable feeling in the stomach, the following has been prescribed :—

Infusion of cascarilla, ℥ vi ;
Bicarbonate of potass, ℥ ii ;
Sedative solution of opium, xx drops ;
Comp. tinct. of lavender, ℥ i. Take two large spoonsful thrice a day, in a state of effervescence, with lemon juice.

This mixture appears to agree with him better than any he has had. His general appearance has improved, and he has had no return of his disease since he came into the hospital.

INDECOROUS CONDUCT AT UNIVERSITY COLLEGE.

To the Editor of THE LANCET.

SIR :—I have been not a little surprised, on attending the lectures of the professor of chemistry at this institution, to find that the students are accustomed, during the beautiful illustration, to make noises of the most unseemly character, stamping, shouting, or hissing, precisely as you hear the visitants in the galleries of Drury-lane and Covent-garden Theatres.

That such conduct is highly indecorous in gentlemen, as well as unfeeling towards Mr. Graham, whose experiments are most successful, and whose attention to and condescension towards the pupils is most marked, is evident. I shall be most happy to forward the names of the individuals who are most active in this employment, if it be continued. I remain, Sir, your obedient servant,

AN OLD STUDENT.

Nov. 1, 1839.

ABSENCE OF THE LEFT KIDNEY.

To the Editor of THE LANCET.

SIR :—On examining the body of Edwin Scanes, aged 1 year and 10 months, who died, on Nov. 17, from mesentric disease, the left kidney was *entirely* absent. There was no appearance of its *ever* having existed. Its place was occupied by the convolutions of the intestines, the spleen lying in its proper situation. The right kidney was very large, weighing four ounces and a drachm (avoirdupois), situated unusually high, its superior end resting on the three inferior ribs, and firmly attached to the diaphragm. The renal capsule presented nothing peculiar. The vessels and ureter were proportionately larger. In this case the left spermatic vein emptied itself into the vena cava. I am, Sir, your obedient servant,

ARTHUR KEMPE, M.R.C.S.

Exeter, Nov. 22, 1839.

LORD STANHOPE AND A FELLOW OF THE COLLEGE OF PHYSICIANS.

LORD STANHOPE, at the last meeting of the Medico-Botanical Society, made some judicious remarks on the present carelessness and apathy of the corporate bodies with reference to the protection of the public health. He had lately had a conversation with a distinguished Fellow of the College of Physicians, with reference to the sale of gingerbread nuts containing dangerous quantities of calomel, and suggested the propriety of establishing some mode by which such pernicious articles might be prevented from being sold. The learned Fellow exclaimed, with petulence, "What! do you want to take the duty off our hands? If it was the duty of the College, Earl Stanhope observed, to guard the public against such poisons, the fact could not be too widely diffused among the public,—how shamefully that duty was neglected.

CORRESPONDENTS.

A correspondent informs us that the *Bristol Mercury*, of December 7th, contains a report of an inquest, held at the Infirmary in Bristol, on the body of a girl who had died on the next day after taking an "overdose" of prussic acid, on which occasion the verdict returned was "Died of disease of the chest." We shall feel obliged to any friend who will forward to us a copy of the newspaper, with or without additional information on the facts of the case; or information on the subject without the paper, if the latter be not procurable.

Letters received from *Mr. John Gorham*, *Mr. George Bodington*, *Mr. J. C. Atkinson*, *Mr. John Brace*, *Mr. Philip Burrows*, *Mr. Rumsey*, *Mr. Denham*, *Dr. Edwin Lankester* (?), and *Mr. W. D. Thompson*.

A Pupil of King's College.—No redress can be obtained. The money has been pocketed, and certainly would not be refunded.

We cannot find room for the rejoinder of *Mr. Hytche*.

We cannot see the point of the letter signed an "*Unexperienced Practitioner*."

The communication of *Dr. Farell* has been in type for the last three weeks, but we have been compelled to postpone its publication from want of room.

Mr. Abraham Banks has forgotten to attach his address to his polite communication.